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Mijne Heeren,

Op 17 October is het buitenlandsch lid onzer afdeeling,

SANTIAGO RAMON Y CAJAL

te Madrid overleden.

RAMON Y CAJAL heeft onze kennis omtrent den bouw van vrijwel het geheele centrale zenuwstelsel en van talrijke onderdeelen van het periphere zenuwstelsel van de vertebraten aanzienlijk verrijkt. Paste hij in het begin de toen recente specifieke neurologische kleurings- en impregnatiemethoden van NISSL en van GOLGI toe, al spoedig vond hij een eigen zilverimpregnatie voor de zenuwelementen, die een uiterst welkome aanvulling van de eerstgenoemde technieken zou blijken te zijn. Met de methode van NISSL werden alle neuronen van een weefselsnede gekleurd, maar van elk neuron slechts het cellichaam, door de methode van GOLGI werd de geheele uitbreiding van een neuron zichtbaar — de cel met dendrieten en neuriet tot in hun verste vertakkingen — maar van elk weefselblokje slechts enkele neuronen. De door CAJAL toegepaste zilverreductie gaf een welkome combinatie van beider voordeelen doordat daarmee alle neuronen in het behandelde gebied elk vrijwel over hun geheele uitbreiding werden gekleurd. Bovendien werden er ook structuurelementen binnen het neuron zichtbaar door gemaakt, n.l. de neurofibrillen.

Gelukkig heeft CAJAL zich niet tot onderzoek met deze eigen techniek beperkt, maar combineerde hij steeds zooveel mogelijk de resultaten van alle genoemde methoden, waaraan later nog weer jongere, ten deele ook door hem zelf ontdekte impregnaties van de glia-elementen werden toegevoegd.

Een nauwgezette, van een fijnen, analytischen geest getuigende beschrijving, steeds door fraaie teekeningen van zijn verwonderlijk mooie preparaten verduidelijkt, heeft een rijke schat van detailkennis omtrent het zenuwstelsel opgeleverd, zoo rijk dat het ondoenlijk is, daarvan hier een overzicht te geven of er zelfs maar een kenmerkenden greep uit te doen. Bij de morphologische beschrijving van een willekeurig gedeelte van het zenuwstelsel heeft, als het op de finesses van den bouw aankomt, heden ten dage in den regel CAJAL of een vertegenwoordiger van zijn school het woord!

In het bijzonder danken wij aan hem een nauwkeuriger kennis omtrent de verbindingen tusschen de vele vezelbanen in de witte stof met de door die banen geïnnerveerde gedeelten der grijze stof, welke verbindingen bij de vóór hem ten dienste staande werkmethoden belangrijk minder goed voor onderzoek toegankelijk waren. Dat deze verbindingen zoo vaak door collateralen — zijtakken van de neurieten — tot stand komen, weten we door CAJAL, die daarmee onze kennis omtrent de centrale reflexbanen niet alleen in vele details, maar ook in beginsel heeft verruimd.

Een der mooiste werken van CAJAL is het onderzoek over het netvlies der werveldieren; hij heeft ons daarin getoond hoe de neuronen in het netvlies samenhangen.

Voorzeker mag CAJAL een der grootste zoo niet de grootste hersenanatoom van zijn tijd genoemd worden. Aan eerbewijzen heeft het hem niet ontbroken; in 1906 werd hem tezamen met GOLGI de Nobelprijs voor Geneeskunde verleend, in zijn land werd hij als een nationale held vereerd.

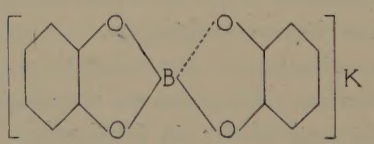
CAJAL bereikte den leeftijd van 84 jaar en heeft ondanks zijne langdurige ziekte tot het laatst toe gewerkt; met hem is een der allergrootsten ten grave gedaald.

Chemistry. — *On the cyclic ethers of pyrocatechol with ketones.* By J. BÖESEKEN and G. SLOOFF.

(Communicated at the meeting of September 29, 1934).

§ 1. In our first communication [1]) on this subject we pointed out that acetone did not behave towards pyrocatechol as might be expected of it, considering the position of the hydroxyl groups in this orthodiphenol.

When one of us started the boric-acid researches, the great increase occasioned by the orthodiphenols on the conductivity of boric acid was a clear indication that the increase of conductivity was closely connected with the easy formation of complexes, possessing a more strongly acid character than the components. For just in the orthodihydroxy derivatives of benzene, of which with great probability might be assumed that the two hydroxyl groups are situated in the plane of the ring, it was to be expected that they would very easily form five-ring systems with boric acid. It should be remembered that the potassiumdipyrocatecholborate has been the first salt, the composition of which has been explained by P. H. HERMANS [2]); the spiranoid constitution shows how easily this five-ring closing takes place:



Meanwhile CHR. v. LOON [3]) had found that some alicyclic cis 1.2 dihydroxy compounds, which highly increased the conductivity of boric acid, very easily produced cyclic acetone compounds.

A very elaborate investigation by P. H. HERMANS [4]) of the equilibria:

$$\text{Diol} + \text{acetone} \gtrless \text{cyclic acetal} + H_2O$$

showed that on the whole the increase of conductivity of these aliphatic

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[1]) Proc. **35**, 170 and 1251 (1932).
[2]) Recueil **42**, 1104.
[3]) Dissertation Delft 1919.
[4]) „ „ 1924.

and alicyclic diols with respect to boric acid runs parallel with the magnitude of these acetone-equilibrium constants.

Diol	Acetone-equilibrium constants at 18°	Increase of conductivity of $^1/_2$ m. $H_3BO_3 + ^1/_2$ m. diol
glycol	0.14	$- 1.0 \times 10^{-6}$
« monochlorhydrine . . .	0.28	+ 7.6 „
propanediol 1.2	0.44	— 0.7 „
propanediol 1.3	0.026	— 4.0 „
glycerol	0.74	+ 11.9 „
cis-cyclopentanediol . . .	> 10.0	+ 149 „
cis-hydrindenediol 1.2. . .	10.7	+ 43 conc. diol = $^1/_{10}$ m.

Comparing the value of the increase of conductivity of equimolecular solutions of pyrocatechol with that of some of the aliphatic diols given here, we might expect not only that a cyclic acetone compound would be formed in the usual way, but also that the equilibrium constant would be very high.

However, if pyrocatechol is dissolved in excess acetone (with 1 % of HCl or H_2SO_4), the formation of a cyclic compound is hardly perceptible. Our first thought, therefore, was that this failing condensation had to be attributed to the peculiar nature of the phenolic hydroxyl groups and that a more vigorous catalyzer would actually serve the purpose. We succeeded indeed in effecting the condensation by means of P_2O_5.

It became apparent, however, that the assumption, already very unsatisfactory in itself, of a practically complete inactivity of conc. H_2SO_4 was untenable with regard to the phenolic hydroxyl group, for:

1. If HCl was used, the cyclic compound was formed as well, though in a very small quantity; the same occurred with conc. H_2SO_4 (1 %). If aqueous acetone was started from, which made the selfcondensation of the acetone insignificant, the cyclic acetone compound amounted to about 0.2 %.

2. Upon addition of a little H_2SO_4 or HCl to the acetone compound, dissolved in acetone, the saponification reaction took place rapidly, until nearly all of the acetone compound had been decomposed.

From this it follows that the reaction may be effected with *all* vigorous catalyzers, but that the equilibrium

$$\text{acetone} + \text{pyrocatechol} \gtrless \text{cyclic ether} + \text{water}$$

is situated in a very unfavourable position.

The success with P_2O_5 should consequently not be attributed to the peculiar nature but to the strongly dehydrating action of this catalyzer, causing the equilibrium to shift strongly to the right. Since neither P_2O_5 nor HPO_3 are soluble in acetone, the self-condensation of acetone falls into the background.

With fuming sulphuric acid also fairly good yields of cyclic ether may be obtained; however, the reaction is then greatly hindered by the self-condensation of the acetone.

§ 2. While the conducive action of P_2O_5 was explained here, it appeared at the same time that the pyrocatechol-acetone equilibrium is very unfavourably situated. Consequently an answer had to be given to the question why this equilibrium is so unfavourable, whereas the position of the hydroxyl groups in the pyrocatechol seems to be, and with regard to the boric-acid indeed *is*, pre-eminently favourable, as appears from the high number for the increase of conductivity.

Let us compare the two actions with that of cis-cyclopentanediol 1.2, of which, owing to the stable position of the five-ring in space, a more favourable position of the hydroxyl groups is to be expected:

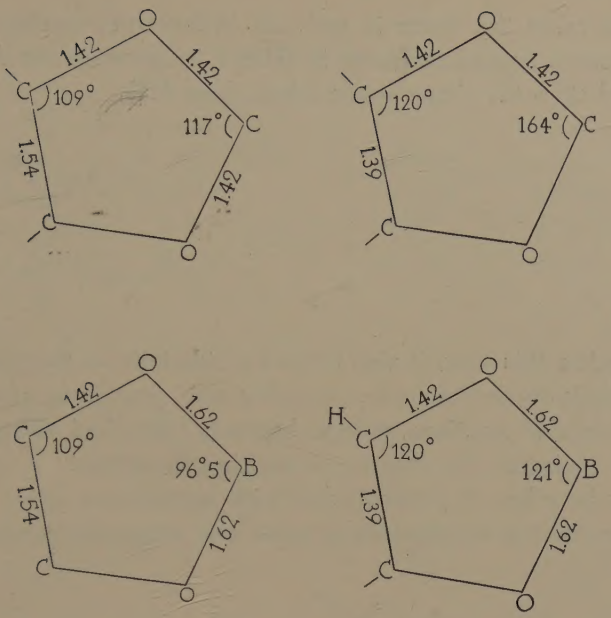
Diol	K_{18} acetone-equilibrium	Δ $^1/_2$ mol + $^1/_2$ m H_3BO_3
cis-cyclopentanediol 1.2 . .	> 10	149×10^{-6}
pyrocatechol	c. 0.001	555×10^{-6}

From these figures it follows that the acetone- and boric-acid methods do not run parallel in case of the aromatic diols.

With regard to the results obtained so far, this should not be explained otherwise than sterically. Taking into consideration that in the benzene-nucleus the six C-atoms, together with the groups united to them, are situated in one plane and that the distance between the various atoms is as follows:

C—C (aliphatic) = 1.54 Å
C—C (aromatic) = 1.39 „
C—O = 1.42 „
B—O (B coordinative 4 val.) = 1.62 „

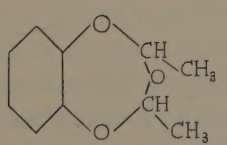
we find the following relations before the closing of the five-rings of cyclopentanediol- and pyrocatechol-acetone and the corresponding boric-acid compounds:



From these relations it becomes apparent that pyrocatechol sterically will much less readily form ring-closing with acetone than with boric acid, even though the hydroxyl groups are situated in the same plane and on the same side of the C-atoms to which they are united. It should also be taken into account here as an unfavourable factor that, owing to the stability of the benzene-ring, the hydroxyl groups are kept immovably in their places. That in the cyclic acetals, when they were once formed, there was tension, could be derived from the exaltation of the refraction, which on an average amounts to 0.68 for the D line.

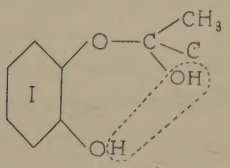
§ 3. In consequence of the unfavourable position of the acetone-equilibrium it is not surprising that the condensation with *aldehydes* did not produce a satisfactory result. Here the self-condensation takes so much the upper hand that a cyclic ether can hardly be obtained.

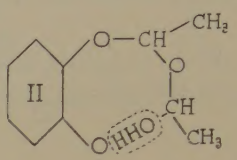
Only with acetaldehyde the important result was obtained that, in addition to a small quantity of the ordinary ether, a cyclic ether was formed which, according to its composition and properties, should be considered as a tension-free seven-ring, originating from one mol. pyrocatechol and *two* mol. acetaldehyde. This substance throws light on



the actual condensation process, in so far as first a mol. aldehyde is added to one of the hydroxyl groups of the pyrocatechol.

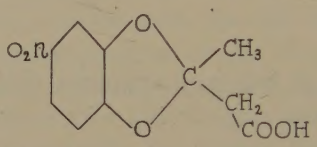
In normal cases the water is split off in five-ring-closing (I). In this particular case a second molecule is shifted in between the first addition product, and then the ring-closing takes place (II).



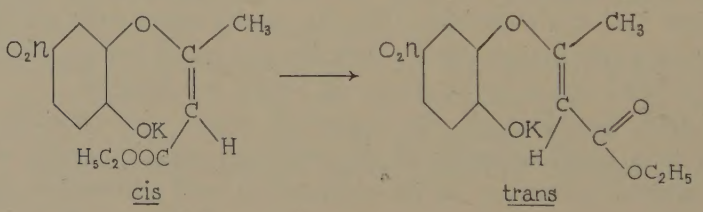


Considered in this way, it also throws a sidelight on the polymerization of acetaldehyde to paraldehyde, probably also consisting of a succession of two dimolecular reactions, where, however, detached valencies have to be assumed, owing to the absence of shifting H-atoms.

In the cyclic ether of pyrocatechol with acetylacetic acid, obtained by saponification of the corresponding ester, one nitro group was introduced

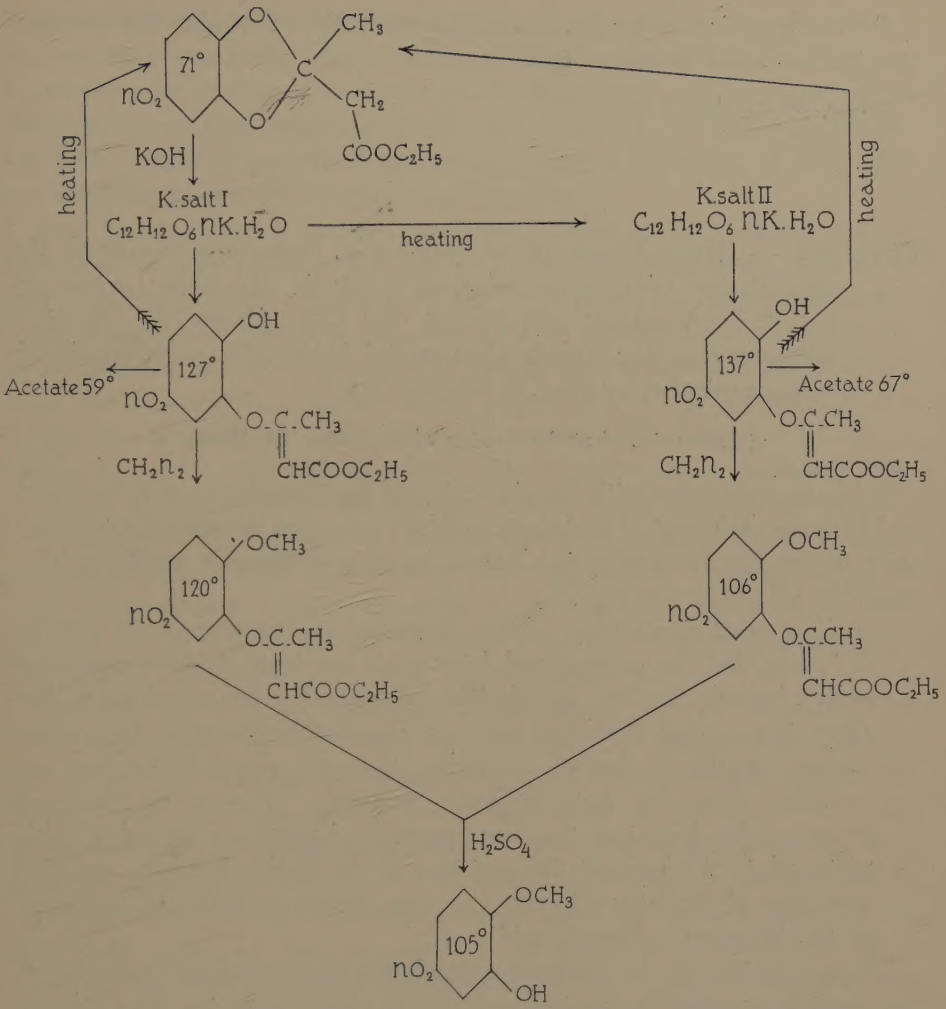


and it was shown that, though this nitro group made the molecule comparatively little asymmetric, the acid still without any difficulty might be split into optical isomers with the aid of cinchonine. Melting-point racemate = 125°. Melting-point dextrorotatory component = 117°.5. $[\alpha]_D^{20}$ = + 41°. It was further found that by the action of alcoholic potassium on the nitro ester two isomeric potassium salts were formed:



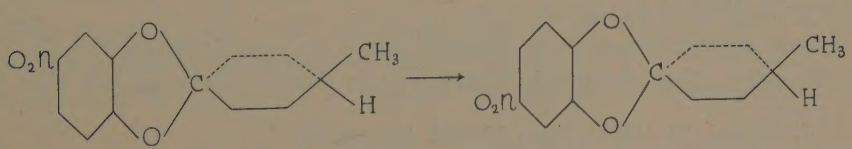
and their derivatives. This is the first case, stated with certainty, of cis-trans-isomerism in an enolether of the acetylacetic ester (see below).

That in case of these potassium salts and the phenols derived from them steric isomers and not position isomers are formed, was proved by methylating the phenols with diazomethane. Two methylethers were actually obtained. By splitting these with an acid, the acetylacetic-ester group was split off and the same nitroguaiacol was obtained. The following diagram gives a survey of the combinations and transpositions:



It may yet be remarked that we obtained a corresponding series of isomers from the free nitropyrocatecholacetylacetic acid.

There has besides been found a particular case of cis-trans-isomerism: in the condensation with p-methylcyclohexanone



a spirane is formed, which after careful nitration produces two isomers, both having the NO_2 group at the position **5**. Although the distance between nitro- and methyl group is fairly considerable and the difference in position of the two groups seems very unimportant, the two isomers differ clearly from each other.

By condensation of the protocatechu ester with o. methylcyclohexanone, two isomers were found as well.

Delft, September 1934.

Chemistry. — *Researches on fat metabolism V. Some experiments on the degradation of unsaturated fatty acids in the living organism.* By P. E. VERKADE and J. VAN DER LEE. (From the chemical laboratory of the Dutch Commercial University at Rotterdam.) (Communicated by Prof. J. BÖESEKEN.)

(Communicated at the meeting of September 29, 1934).

§ 1. In connection with the discussion of the investigations of LEATHES and MEYER—WEDELL [1]) and those of HARTLEY [2]) on desaturation of fatty acids in the liver, LEATHES and RAPER, in their wellknown monograph [3]), make the following remark:

> "The exact significance of the process of desaturation is not known. It is true that the unsaturated fatty acids are more liable to oxidative attack because of their double linkages than the saturated acids, but there is no proof that this accepted fact of the chemical laboratory applies equally to that of the living organism where oxidations are brought about by very different methods. It may be accepted provisionally, however, that the unsaturated acids are probably liable to attack in the body at the positions of the double linkages and that a rupture of the fatty acid chain at these points precedes the oxidation of the fragments so produced."

The probability of the mode of degradation of unsaturated and also of saturated fatty acids, briefly indicated in the above quotation, was already put forward as early as 1913 by LEATHES in the first edition of the above-mentioned monograph. The ideas expressed there have been amply discussed and partially contradicted by BLOOR [4]). We need not discuss this paper here. BLOOR's assertion that this mode of degradation was already suggested by LEATHES in a lecture delivered before the Harvey Society in 1909 [5]) is incorrect.

Recent researches, such as those of SINCLAIR [6]) and those on the nature of the fatty acids present in sheep or pig liver [7]), have shown that the

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[1]) J. Physiol. **38** (1909), Proc. Physiol. Soc., p. 38.

[2]) Ibid. **38**, 353 (1909).

[3]) The Fats (LONGMANS, GREEN and Co., London, 1925), p. 194.

[4]) J. metabolic Research **4**, 549 (1923).

[5]) Lancet, 1909, I, 593.

[6]) J. Biol. Chem. **95**, 393; **96**, 103 (1932); **100**, LXXXVII (1932).

[7]) TURNER, Biochem. J. **24**, 1327 (1930); CHANNON, IRVING and SMITH, ibid. **28**, 840 (1934).

basis on which the desaturation hypothesis of LEATHES rests is much weaker than was fairly generally accepted before. At present there is no proof whatever for the correctness of this hypothesis and it seems indeed doubtful to us that desaturation of fatty acids *in the sense meant by* LEATHES should take place in the living organism. However, in this paper we shall not discuss this problem any further.

It is, however, in our opinion, quite worth while to go carefully into the question whether perhaps the double bonds of unsaturated fatty acids may indeed be points of attack for the degradation of these acids in the living organism. It seems to us to be not at all improbable *a priori* that the degradation of a certain part of the unsaturated component acids of our food fats begins with a rupture of the carbon chain at the positions of the double bonds which are already present in these acids. The work discussed below is to be considered as a very first contribution in answer to this question. Encouraged by the success of our simple *in vivo* experiments with triglycerides derived from normal saturated fatty acids [8]), we have now made some similar experiments with triglycerides, containing unsaturated fatty acids as component acids, in the hope that we might perhaps succeed in isolating from the urine of the subjects partial degradation products of these fatty acids, pointing in this direction.

LEATHES, and LEATHES and RAPER respectively, assume the rupture of the unsaturated fatty acid chains to occur in such a manner that carboxyl groups are formed at the positions of the carbon atoms, forming the double bonds; oleic acid, for example, would thus break up into azelaic acid and pelargonic acid. In our opinion, this is *a priori* by no means certain. It is possible, for example, — work of QUAGLIARIELLO and his collaborators [9]) points in this direction — that the first phase of the one-sided β-oxidation is a desaturation of the fatty acid at the $\alpha\beta$-position, while it is often assumed that in this mode of degradation the two carbon atoms are split off in the form of acetic acid; thus, if the one-sided β-oxidation does indeed proceed in this way, the degradation products expected by the above-mentioned investigators are not formed on the rupture of the carbon chain of an $\alpha\beta$-unsaturated fatty acid. It does not seem impossible to us that the positions of the double bonds may in certain cases influence the nature of the eventual degradation products. Theoretical speculations on the possible course of the mode of degradation of unsaturated fatty acids, under discussion here, are meanwhile of very little use. OPPENHEIMER [10]), who as well as WIELAND [11]), accepts the possibility of such a rupture of the

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[8]) VERKADE, ELZAS, VAN DER LEE, Miss DE WOLFF, Mrs. VERKADE-SANDBERGEN and VAN DER SANDE, Z. Physiol. Chem. **215**, 225 (1933); VERKADE and VAN DER LEE, Biochem. J. **28**, 31 (1934); Z. Physiol. Chem. **225**, 230 (1934); **227**, 213 (1934).

[9]) MAZZA and ZUMMO, Rend. R. Accad. Naz. Lincei (6) **18**, 461 (1933); QUAGLIARIELLO, Progr. scient. 9e Congr. intern. de Chimie, Madrid, 1934, p. 98.

[10]) Chem. Grundlagen der Lebensvorgänge (GEORG THIEME, Leipzig), 1933, p. 143.

[11]) Über den Verlauf der Oxydationsvorgänge (FERD. ENKE, Stuttgart), 1933, p. 44.

carbon chain, correctly confines himself to the remark that two substances would then be formed from, for example, oleic acid, each with a chain of nine carbon atoms. Experimental work will have to show whether such a rupture of the chain of unsaturated fatty acids takes place in the living organism and which fission products may then be formed.

The problem of the catabolism of unsaturated fatty acids in the human organism is for various reasons of the greatest importance. Such fatty acids generally form a very considerable percentage of the component acids of our food fats. A study of the constitution of the unsaturated fatty acids occurring as such — oleic acid, linolic acid, linolenic acid, vaccenic acid, palmitoleic acid, erucic acid, etc. — shows at once that for these acids the degradation principles, known from the saturated fatty acids, *viz.* the β- and the ω-ovidation, by no means work out. If the β-oxidation should indeed take place via an $\alpha\beta$-unsaturated acid as intermediate product, nevertheless the double bonds, already present in the unsaturated component acids of our food fats, lie mostly in such positions that they render the unhindered continuation of the process of β-oxidation impossible. It is certainly a remarkable fact that until now, at least as far as we know, attention has never been drawn to this point.

§ 2. *Triundecylenin.*

The very pure sample of triundecylenin used by us (saponification value 284.1, calculated 285.1 ; iodine value 128.5, calculated 129.0) was obtained by esterification of carefully purified undecylenic acid (setting-point 23.6°; equivalent weight 185.0, calculated 184.16 ; iodine value 137.5, calculated 137.8) with glycerol in the presence of some zinc as a catalyst[12]) and refined according to the method used commercially.

OZAKI[13]) had already prepared this triglyceride before by esterification of the components by means of Twitchell acid and used it for feeding experiments on rats. It then appeared that within 2—3 hours after administration of a diet, consisting of a fat free basal diet with 5 or 2.7 % triundecylenin, the experimental animals died showing symptoms of paralysis. Upon examination in the same way the triundecylin, obtained by catalytic reduction of the triundecylenin used, appeared on the other hand, to be not only not poisonous but even nutritious. From this fact OZAKI draws the conclusion that "die giftigen Eigenschaften des Triundecylenins deshalb auf die ungesättigte Bindung im Molekül zurück zu führen sind".

Previous researches[8]) had shown us that in the series of simple triglycerides of the normal saturated fatty acids the terms with term-number 9 (tricaprin) and 10 (triundecylin) are by far the most strongly diacidogenic. Consequently it was very attractive to us also to include in

[12]) Cf. VERKADE, VAN DER LEE and Miss MEERBURG, Rec. trav. chim. **51**, 850 (1932).

[13]) Biochem. Zeitschr. **177**, 156 (1926).

our researches a corresponding *un*saturated glyceride, *viz.* triundecylenin. For our researches on fat metabolism, published as yet, we constantly made use of human subjects; we have only very recently started experiments on animals in various directions. We therefore thought it advisable to make use of *human subjects* also in the planned experiments with triundecylenin. However, as a consequence of the observations by OZAKI on the deleterious action of this triglyceride, we had to take great care with such experiments and, at any rate in the beginning, only we ourselves might act as subjects. One of us partook of 15 g of triundecylenin, together with a liberal amount of carbohydrate, without externally perceptible injurious consequences; with the other one, on the contrary, the administration of 25 g of this triglyceride occasioned a slight nephritis (acute and marked proteinuria; in the sediment numerous hyaline and some granular cylinders, as well as fairly many red corpuscles). We therefore thought it advisable to give up further similar experiments.

It may be incidentally observed that this remarkable injurious action of triundecylenin on the kidneys should be investigated more closely in various directions. We hope soon to find an opportunity to do this.

To both subjects the triundecylenin was administered together with a liberal amount of carbohydrate. The addition of carbohydrate to the fat took place only because in our previous work on fat metabolism we had observed its stimulating action on the production of dioic acid acidosis and diaciduria; we now hoped to be able in this way to stimulate also in the case of triundecylenin the accumulation of partial degradation products of undecylenic acid in the blood and their excretion in the urine. The urine was collected during about 24 hours after the administration of the fat. *In both cases we succeeded with perfect certainty in isolating a very small amount of sebacic acid.* The method of working up the urine need not be described here in detail, as it agreed in principle with that, already communicated in previous papers.

From the urine of the subject V., to whom 25 g of triundecylenin was administered, about *10 mg* of crude sebacic acid with a melting-point of 122—127° was obtained. The mixed melting-point with a sebacic acid specimen, obtained in the usual manner from castor oil and melting at 132.5—133.5°, amounted to 125—131°. This substance was recrystallised from hot water and analysed:

2.500 mg → 5.38 mg CO_2 and 1.90 mg H_2O; C **58.7** % H **8.5** %
Calculated for $C_{10}H_{18}O_4$: C 59.36 % H 8.98 %.

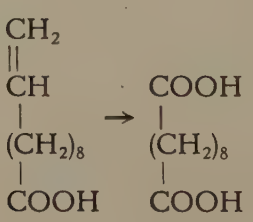
For the subject V. D. L., to whom *15 g* of triundecylenin had been administered, the amount of crude sebacic acid was about *20 mg*. After crystallisation from benzene and then from water, the product melted at 128—131°; mixed melting-point 129—133°. This substance was analysed:

3.991 mg → 8.74 mg CO_2 and 3.13 mg H_2O; C **59.7** % H **8.8** %
3.235 mg → 7.08 mg CO_2 and 2.54 mg H_2O; C **59.7** % H **8.8** %

It is to be regretted that because of the deleterious action of triundecylenin on the kidneys experiments with the administration of larger doses of this fat were inadmissible; without doubt larger amounts of sebacic acid would then have been excreted in the urine.

In judging this result, the following should be taken into account. Various investigators (BAER and BLUM; MORI; FLASCHENTRÄGER; SMITH) have proved that dogs and rabbits are able to burn higher normal saturated dicarboxylic acids. Sebacic acid was examined in this respect by BAER and BLUM [14]) with phlorhizin diabetic dogs and by FLASCHENTRÄGER [15]). By the latter, for example, 10 g of sebacic acid in the form of its sodium salt were subcutaneously injected into a healthy dog, weighing 21.2 kg, in amounts of 0.5 g twice a day; from the urine, collected throughout a fortnight, 61 % of the injected sebacic acid could be recovered, so that 39 % had been burnt, unless, of course, retention of the acid then still took place, which is indeed highly improbable. It should be remarked here that such injection experiments may easily give a too unfavourable impression of the combustibility of a substance, since at each injection the organism is suddenly flooded with this substance. Experiments by ANDERSEN [16]) further showed that adipic acid, administered *per os*, can be burnt also in the human body. The obvious conclusion that this will thus be the case with sebacic acid as well finds an indirect support in the results of researches recently published by us [17]). After administration *per os* of tricaprin, the urine of the subjects appeared to contain some suberic and adipic acid in addition to large amounts of sebacic acid; it is highly probable, not to say certain, that the first-mentioned acids were at any rate partly formed by degradation of sebacic acid. From all this it may certainly be concluded that the small amounts of sebacic acid, isolated by us from the urine after administration of triundecylenin, are in any case only part of those originally formed.

There is, therefore, according to us, not the slightest possible doubt that *in the degradation of triundecylenin in the human organism sebacic acid is formed as an intermediate product. This is only possible if the double bond of undecylenic acid is a point of attack in the degradation of this acid*:



[14]) HOFMEISTERs Beitr. **11**, 101 (1908).
[15]) Z. Physiol. Chem. **159**, 297 (1927).
[16]) Cf. FLASCHENTRÄGER, loc. cit., p. 299.
[17]) VERKADE and VAN DER LEE, Z. Physiol. Chem. **225**, 230 (1934).

§ 3. *Olive oil and rape oil.*

We included the first mentioned fat in our researches in view of its very high content of *oleic acid* as component acid. This acid is partly present as triolein, partly in the form of mixed triglycerides; in the cold pressed olive oil used by us ("extra vierge" of Etabl. C. Castel et Fils in Nice) triolein is decidedly the principal component. However, as follows from the results of one of our previous papers [18]), combined with not yet published results of experiments with synthetic mixed triglycerides, it is indifferent whether we have to deal with either simple or mixed triglycerides.

Considerable amounts of olive oil were administered fairly rapidly to the healthy subjects V. D. L. and V. — *e.g.* 150 g in three equal portions at about equal time intervals in the course of 10 hours — and together with a liberal amount of carbohydrate. While working up the urines, collected during 24 hours after administration of the first portion of fat, we noticed nothing of the presence of any substance, which might be considered as a partial degradation product of oleic acid; it is unnecessary to describe the method of working up the urine.

The fatty oils from the family Cruciferae are characterized by a high percentage (generally about 40—50 %) of *erucic acid* as a component acid; this is probably mainly present in the form of mixed triglycerides [19]).

To the healthy, sober subject V. D. L. we administered 110 g of oil of Brassica campestris L. in three practically equal portions at about equal time intervals in the course of 8 hours, together with a liberal amount of carbohydrate. While working up the urine, collected again during 24 hours, we did not find any indication in this case either of the presence of any substance, which might be regarded as a partial degradation product of erucic acid.

It stands to reason that from the negative results of our experiments with olive oil and rape oil the conclusion may not be drawn that the double bond of oleic acid or erucic acid is not a point of attack for the degradation of these acids. It is indeed quite well conceivable that under the conditions of our experiments the degradation products, formed by fission of the molecule at the position of the double bond, are not accumulated in the blood to a sufficient extent to lead to their excretion in the urine. Why this is then not the case after administration of large quantities of these fatty oils at a great rate, whereas it occurs after administration of only small amounts of triundecylenin (comp. § 2), is a question which lends itself splendidly to theoretical speculations but which, at the present stage of our work, is unsuitable for an exact discussion.

§ 4. For the sake of completeness the investigations of SMITH on the catabolism of oleic acid have yet to be discussed here. Indeed, in his

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[18]) VERKADE and VAN DER LEE, Z. Physiol. Chem. **225**, 230 (1934).

[19]) Cf. TÄUFEL and BAUSCHINGER, Z. Untersuch. Lebensm. **56**, 265 (1928).

second paper on this subject [20]) this author arrives at the following general conclusion:

> "The accumulating evidence thus seems to indicate that the metabolic oxidation of fatty acids proceeds in a manner independent of the presence or position of a double bond".

Apart from the fact that our experiments with triundecylenin, described in § 2, positively lead to the exactly opposite conclusion, much can be said against the argumentation employed by SMITH. In the first place all experiments made by him are based on the already mentioned hypothesis that azelaic and pelargonic acid should be produced on the rupture of the carbon chain of oleic acid at the position of the double bond. At most SMITH might consequently have concluded that *such* a rupture does not take place in the organism. The far more general conclusion cited above may not be drawn from the results of his experiments; for, as we incidentally pointed out in § 1, we need not by any means *a priori* and of necessity accept that the mode of fission of oleic acid, assumed by SMITH, is the only possible one.

SMITH's evidence consists of the results of feeding experiments on dogs. In the course of 6 days these dogs had administered to them in their food 20—45 g of azelaic acid in the form of its sodium salt, *i.e.* "amounts smaller than would be produced normally in the body if oxidation took place at the unsaturated linkages". Of course, what is meant here by SMITH is: smaller than the amount, which would be formed intermediarily if all unsaturated food fatty acids were to be *exclusively* attacked in the way suggested by LEATHES and RAPER. This, however, is an exceedingly rash suggestion. It is indeed very likely that the unsaturated fatty acids are catabolised in various ways; for the saturated fatty acids this has already been pointed out with certainty, merely as a result of our work. On an average about 60 % of the administered azelaic acid appeared to be excreted in the urine. During the various periods of 6 days the excreted amount varied greatly; it amounted to at most 91 % of 31.0 g and at the least 29 % of 35.3 g. It is this on the whole rather bad utilization of azelaic acid administered *per os*, which made SMITH doubt the correctness of the hypothesis in question and even made him draw the general conclusion cited above. This doubt is a result of SMITH's highly curious conception [21]) that "if the short chain mono- and dibasic acids, formed in the laboratory oxidation of the unsaturated acids, are fed to animals and are utilized, the theory stands; if they are not burned, the theory lacks direct support and falls". (!) It is unnecessary to point out here that SMITH made use of the results of his feeding experiments with azelaic acid for unpermitted interpretations.

§ 5. The researches discussed above have, to express it cautiously,

[20]) J. Biol. Chem. **103**, 531 (1933).
[21]) Ibid. **67**, Proc. Am. Soc. Biol. Chem. p. XXVII (1926).

produced a clear indication *that the double bonds of unsaturated fatty acids may indeed be points of attack for the degradation of these acids in the living organism, and that also in this mode of degradation normal saturated dicarboxylic acids are formed as intermediate products, or at any rate may be formed in certain cases.* It becomes constantly more apparent that the latter acids play a very important part in fat metabolism.

A further investigation into the catabolism of the unsaturated fatty acids is no doubt highly necessary.

We wish to make grateful acknowledgment to the HOOGEWERFF-Fonds of the Hague and the ERASMUS-Stichting of Rotterdam for a grant-in-aid of this work.

Astronomy. — *On the foundations of the theory of relativity, with special reference to the theory of the expanding universe.* By W. DE SITTER.

(Communicated at the meeting of October 27, 1934).

The restricted theory of relativity was based on the postulate of invariance of the laws of nature for LORENTZ-transformations, and the line-element of space-time accordingly was taken to be

$$ds^2 = -dx_1^2 - dx_2^2 - dx_3^2 + c^2\,dt^2 . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In the general theory of relativity the invariance is postulated for *all* transformations of coordinates, and the line-element (1) is replaced by the more general riemannian form

$$ds^2 = g_{\alpha\beta}\,dx_\alpha\,dx_\beta , \qquad (\alpha, \beta = 1 \ldots 4) \quad (2)$$

with the restriction, however, that it shall be of index + 1, and therefore *locally* reducible to the galilean form (1). In other words the fundamental hypothesis is:

I. The laws of nature are expressible as equations between tensors in a continuous four-dimensional manyfold, the line-element (2) of which is reducible to the form

$$ds^2 = -a^2\,d\sigma^2 + v^2\,dt^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

a and v being real functions of the four variables x_1, x_2, x_3, x_4, and

$$d\sigma^2 = \gamma_{pq}\,dx_p\,dx_q \qquad (p, q = 1, 2, 3)$$

being a positive definite quadratic form of the differentials of the three variables x_1, x_2, x_3.

In the popular expositions of the theory too much stress has as a rule

been laid on the *relativity* of space and time, which is a consequence of this general invariance. It has even been proposed to replace the principle of invariance by other principles expressing this relativity, such as the so-called "cosmological principle", which asserts that statistically the world pictures of two different observers must be the same. We have, however, no means of communicating with other observers, situated on faraway stars, or moving with excessive velocities, such as that of a β-particle, and the building of world structures on the supposed experiences of these fictitious observers is equivalent to the introduction in disguise of certain specific assumptions regarding the interpretation of our own observations. The task to remove this disguise, and to ascertain what exactly these assumptions are, is not always an easy one. It seems better to introduce our assumptions explicitly and in clear language.

The hypothesis I is the necessary foundation of an intelligent description of nature. The question whether the postulated four-dimensionality and continuity are properties of the outside world, or of our way of describing it, can be left out of consideration for the moment. [As to the continuity, I may remark that we are here considering macroscopic phenomena only.] Also the possibility can perhaps not be excluded a priori that a further development of the theory with a view to make it embrace more phenomena, may lead to the adoption of a non-riemannian line-element. But, however this may be, we are at present entitled to postulate our "hypothesis" I, and to consider it simply as a precept how to introduce coordinates into our description of the world. It implies, of course, the axiom that the laws of nature are the same always and everywhere. In particular, with a view to the observations on which our knowledge of the expansion of the universe is based, the mechanism that produces the H and K lines of ionised calcium in a distant nebula is the same as in our laboratory.

The extrapolation outside our immediate neighbourhood is based on the following assumptions or hypotheses:

II. The tracks of material particles and photons (world lines) are geodesics in the four-dimensional continuum.

This again is not really an assumption regarding the nature of these tracks, but a precept for the construction of the space-time geometry by which we represent them.

III. In the universe considered on a large scale there is in every region of sufficient extent a definite *mean* motion, the deviations of the actual motions of the individual material particles from this mean being unsystematic.

WEYL has been the first to point out that some such postulate regarding the coherency of matter is unavoidable. If we adopt our hypothesis III, we can at each point choose the variable x_4 along the tangent to the

geodesic representing this mean motion, and call this the "time" t, and we can choose the other variables so that the "spaces" $t = constant$ are orthogonal to this time. It should be emphasized that the "cosmic time" thus defined is not introduced by the observer or the theoriser, but depends on the actual state of motion of the matter in the universe. Space and time are therefore not mathematical concepts, which can be chosen at will, but are determined by the physical status of matter and energy according to the precepts I and II.

On the other hand this definition of "cosmic time" is a statistical one, and it consequently is only determined within certain limits of uncertainty, depending on the spread of the individual motions relative to the mean. It shares this statistical character with most physical quantities, like the temperature of a gas, etc. The time used in our physical theories, or in the planetary motions, is the proper time of the earth, or of the sun. The deviation of these different times from each other is negligibly small, and, according to our hypothesis III, their deviation from the "cosmic time" is also small.

In the universe considered on a large scale our hypothesis III naturally suggests the further hypothesis of three-dimensional isotropy, or stated explicitly:

IV. In any three-dimensional space $t = constant$ the spacial directions around any point are physically indistinguishable.

This, of course, like III, is a statistical postulate, valid only for the average distribution of matter over a large region, but not for the particular motions of individual particles, which are expressly excluded from consideration.

It can be proved [1]) that, if the hypotheses I to IV are true, the line-element is reducible to the form

$$ds^2 = -R^2 d\sigma_k^2 + c^2 dt^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where $d\sigma_k$ is the line-element of a three-dimensional space of constant curvature ($k = +1$, 0 or -1), c being a constant and R a function of the time t alone (which might be a constant). It is thus seen that isotropy implies homogeneity.

The three-dimensional line-element can be written in the form

$$d\sigma_k^2 = d\chi^2 + b^2 (d\psi^2 + sin^2 \psi \, d\vartheta^2), \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where $b = sin\ \chi$, χ, $sinh\ \chi$ for $k = +1$, 0, -1 respectively. An observer interpreting his observations does, however, not use the radius vector χ of the unit space, but a galilean system of coordinates for his particular position in space and time, of which the radius vector is given by

$$dr = R\, d\chi.$$

[1]) See e. g. ROBERTSON, *Proc. Nat. Acad. Sci. Washington*, XV, p. 822, 1929. It should be remarked that strictly speaking it is sufficient to make the hypothesis IV for *one* space $t = t_0$, and for *two* of its points.

This is not a complete differential, and to define the radius vector, or distance, outside our immediate neighbourhood the path of integration must be prescribed, or the distance must be otherwise defined[1]). The distance that can be astronomically measured by the parallax, the angular diameter or the intensity of light, is in the three cases

$$\begin{array}{lll} k=1 & k=0 & k=-1 \\ r=R\sin\chi & r=R\chi & r=R\sinh\chi. \end{array}$$

The function R of t is not known. We put

$$h=\frac{1}{R}\frac{dR}{c\,dt}. \quad \ldots\ldots\ldots \quad (6)$$

This h is the coefficient of expansion of the universe. It also is an unknown function of the time.

It follows from (4) that the relation between the observed and emitted wavelengths, λ and λ_0 respectively, is

$$\frac{\lambda-\lambda_0}{\lambda_0}=\frac{R-R_0}{R_0}, \quad \ldots\ldots\ldots \quad (7)$$

which for small displacements gives, by DOPPLER's principle, the velocity-distance relation

$$\frac{V}{c}\sim r\,h \quad \ldots\ldots\ldots \quad (8)$$

From the observations of the spectra of spiral nebulae it follows that at the present moment h is positive, and its value is approximately

$$h=\tfrac{1}{2}\,.\,10^{-27}\ cm^{-1}.$$

At present this is all that these observations can teach us, but it seems not impossible that with the increase of optical power of our telescopes we shall ultimately be able to see so far into space, or, which comes to the same thing, so far back into time, that it may become possible to determine also the differential quotient dh/cdt and the curvature $\varepsilon=k/R^2$.

It should be remarked that the distance-velocity relation (8) is thus derived from the hypotheses I to IV only, without making use of either the field-equations, or the concept of "velocity" as a change of coordinates.

The field-equations

$$G_{\mu\nu}-\tfrac{1}{2}\,g_{\mu\nu}\,G+\lambda g_{\mu\nu}+\varkappa T_{\mu\nu}=0 \quad \ldots\ldots \quad (9)$$

express the manner in which the metric of the physical universe, i. e. the metric tensor $g_{\mu\nu}$, is determined by the matter, i. e. the material tensor $T_{\mu\nu}$. The equations (9) are equivalent to the law of gravitation, and thus "gravitation" may be defined as that property of matter by which it determines the metric of time-space. It only becomes of importance when

[1]) See *B. A. N.* VII, No. 261, p. 205, 1934.

we consider the universe on a small scale, concentrating our attention on the motions of individual particles instead of on the statistical average of a large number of particles. By analogy we may define "inertia" as that property of matter which is expressed by the hypotheses I to IV (especially II), and we can thus say that *the expansion of the universe is a direct consequence of the law of inertia.*

This statement is, however, only qualitatively correct. Quantitatively the numerical values of the coefficient of expansion and its differential quotient are connected with the density ϱ and pressure p of matter (and radiation) by means of the field-equations, which give

$$\left.\begin{aligned} h^2 &= \tfrac{1}{3}(\lambda + \varkappa\varrho) - \varepsilon \\ \frac{dh}{c\,dt} &= \varepsilon - \tfrac{1}{2}\varkappa(\varrho + p). \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (10)$$

WEYL, and after him EDDINGTON, have emphasized the necessity that there must be woven into the structure of the world a standard of length (and time) making possible the comparison of lengths at different points in space-time. According to MACH's principle this natural standard must be of material origin. It appears to me that it is provided by the equations of conservation of mass (energy) and momentum

$$div\, T_{\alpha\beta} = 0,$$

which give

$$\frac{d\varrho}{c\,dt} + \frac{3}{R}\frac{dR}{c\,dt}(\varrho + p) = 0 \quad \ldots\ldots\ldots \quad (11)$$

In the simplest case (which is, however, a very good approximation in the actual world) that the pressure is neglected, the integral is

$$\varkappa\varrho = \frac{3R_1}{R^3} \quad \ldots\ldots\ldots \quad (12)$$

If the universe is finite ($k = +1$), so that it has a total mass, this mass is $M = 3\pi^2 R_1/\varkappa$. In other cases the integral is not so simple, but it still contains the same constant of integration R_1, which is still a measure of the material contents of the universe, and, since it has the dimension of a length, it is the natural unit of length and time. In empty universes, containing no matter, R_1 is zero: there is no natural material unit of length, and none is required. Using EDDINGTON's suggestive metaphor [1]) we can say that R_1 is the "real Gulliver", and evidently no Gulliver is required when there are no islands for him to visit. The discussion of empty universes is not a physical problem, but an exercise in geometry.

It should be pointed out that the equation (11) is also independent of the field-equations (9), and depends only on the assumption that the line-element has the form (4).

1) *The expanding universe*, chapter IV, Cambridge, 1933.

Anthropology. — *The Central Asiatic barrier and the distribution and associated occurrence of cephalic index peaks in Asiatic-European races.* By C. U. ARIËNS KAPPERS.

(Communicated at the meeting of October 27, 1934).

In previous communications in these Proceedings (1931, '32 and '33), and in an "Introduction to the Anthropology of the Near-East in ancient and recent times" [1]) I called the attention to the comparison of frequency curves instead of averages for the establishment of homologous head indices of various peoples.

This method is the more advisable as no groups whatever are pure, and their different admixtures with other races [2]) may influence the averages to such an extent that they obscure homologous groups which may be revealed by curve comparison.

Encouraged by the result of this way of investigation I have applied the same method to more peoples of the Eurasian mainland confining myself north of the 30th parallel, for Europe North of the 44th, thus excluding the mediterranean region that has been partly dealt with in the above quoted book.

The result of this work is not only interesting on account of the relations it reveals, but also on account of the fact that it shows an interesting mutability in the index, the knowledge of which may be valuable as it may give us a clue to developmental features of the human skull.

Before dealing with the index curves I may remind that the Asiatic mainland is divided diagonally by the mountainous ranges of the Hindoo-Koosh, Pamir, Tien Shan, Altai Tag, and Sajan mountains in a Western and Eastern part, the importance of which for racial distributions has been emphasized by various authors, especially by VON EICKSTEDT [3]). The Himalayan and Trans-Himalayan barrier has a similar importance for the distribution in Southern and Northern direction, but the distribution South of the Himalayan barrier will not be considered in this paper. What concerns us here is the indicial distribution North of the Himalayan barrier.

In Table I I plotted some of the most important groups of indices, arranging the curves so that those to the right of the dividing line refer to

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[1]) N.V. Noord-Hollandsche Uitgevers Maatschappij, Amsterdam, 1934.

[2]) Eventually also different mutations of the head index; see p. 606.

[3]) VON EICKSTEDT, Rassenkunde und Rassengeschichte der Menschheit, Enke, Stuttgart 1933.

peoples East, those to the left to peoples West of the mountainous barrier mentioned above.

What strikes us first of all is the prevalence of the 79 and 81 (82) combination East of the barrier and the frequent occurrence of the 83 (84)—86 (87) combination to the West.

Several peaks in the curves show a striking constancy with the allowance of a variation of one entity in the peak. This slight difference may be due to differences in the personal ways of measuring, to the differences of instruments, but also to real variations falling within this range.

A typical example is seen with the Turks measured by VON LUSCHAN and those measured by myself (Table I, N°. 6), the highest peak in VON LUSCHAN's curve being at 86, in my curve at 87. Such a difference does not seem to have great importance for the problem that concerns us, as a comparison of the various curves suggests. It may be an expression of variability falling in the same category as the variability found by BOAS in his studies of the American born Jews and American born Italians in which cases the difference (expressed in averages by BOAS) also involved about one entity.

These *variations* should be well distinguished from the greater changes or *mutations* that will be discussed below.

First dealing with the Western side of the barrier I must call the attention to the curves on Table I, fig. N°. 1, showing in a thin continuous line the cephalic indices of 126 males of Khotan and Turfan, Taklamakan border cities in the Pamirs[1]), registered by Sir AUREL STEIN[2]), and published by JOYCE[3]). In the thick continuous curve to these indices are added those of the 37 ♂ Kirghises recorded by JOYCE, and of the 242 ♂ Kirghises of Ferghanah, Kohistan and Kouldja recorded by DE UJFALVY[4]). From the result it appears that these central Asiatic brachy- and hyper-brachycephals are closely related as far as their index is concerned[5]), the chief peaks being at 83 and 87.

Underneath these central Asiatics I put 176 male Armenians from New Julfa (near Isphahan) in Persia, measured by Dr. KRISCHNER[6]). Apparently the same peaks occur here. The additional 78 peak (failing with the Kirghises) is due to Persian admixture (cf Table II N° 12) as proved before[6]), and need not concern us at the moment.

Again the same index peaks are found with the population of the Alouite

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[1]) In addition to these a large number of 78 index people occurs in Chinese Turkestan. They are registered in table II, fig. 12, dotted line, together with the Kurds (continuous line) and Persians (crossed line) and will be later discussed.

[2]) Sir AUREL STEIN, Serindia, Vol. III, Oxford, 1924.

[3]) JOYCE, Notes on the physical anthropology of Chinese Turkestan and the Pamirs. Journ. of the Anthrop. Instit., Vol. **56**, 1926.

[4]) DE UJFALVY, Expédition scientifique française en Russie, en Sibérie et dans le Turkestan, Vol. I and II, Lerroux, Paris, 1878 and 1879.

[5]) The same may hold good for the 56 female Yakuts measured by Mrs. JOCHELSON BRODSKY, indicated by the dotted curve, and so different in their indicial relation from the male Yakuts, measured by HECKER (Table I, N°. 11 dotted line, resembling the Jakughir curve).

[6]) Dr. and Mrs. KRISCHNER, The anthropology of Mesopotamia and Persia. C. The anthropology of Persia. These Proceed., Vol. **35**, 1932.

mountains, the Ansarieh, living in Syria, North of the Lebanon, measured by myself (145), and by CHANTRE [1]) (48): Table I N°. 3.

A curious fact, however, is that facially the people registered in curve N°. 1 on one hand and curve N°. 2 and 3 on the other hand do not at all resemble each other, the former showing frequently (not always) mongoloid, the latter semitic or, better, armenoid features. This is a clear proof of the fact, also emphasized by TSCHEPOURKOWSKY (l.c. infra), MORANT [2]), KOHLBRUGGE [3]), and RIPLEY [4]), that facial characteristics, however important for establishing relations in a narrower sense, have not nearly the same value for the racial relations in a wider sense.

The people registered in textfigure 1: 181 male Lebanese measured by myself (continuous line) and the 280 male Druses (200 measured by SHANKLIN, 80 by myself; dotted line) show very homologous curves differing only in the subbrachycephalic admixture, this admixture with the Lebanese having a peak at 80, with the Druses at 82.

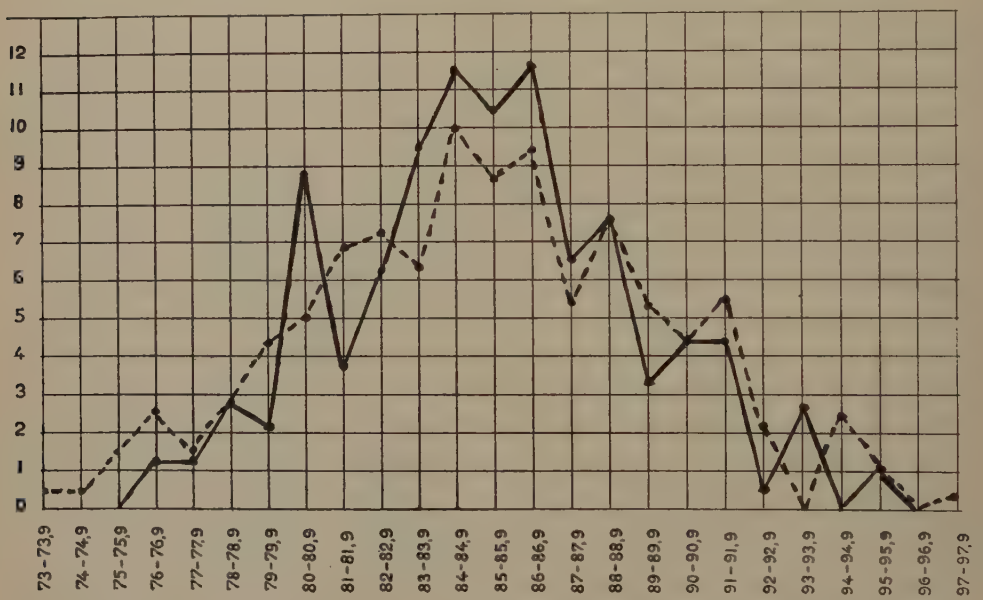

Fig. 1. Continuous line: 181 male Lebanese, measured by the author. Dotted line: 280 male Druses (200 measured by Dr. SHANKLIN, 80 by the author), expressed in percentages.

With these peoples, both chiefly living in the Lebanon mountains, the highest index peaks are not at 83 and 87 but at 84 and 86. As stated above, such differences fall in the usual range of variation, the same phenomenon being seen with the hyperbrachycephalic index of my Turks and those measured by VON LUSCHAN (Table I, N°. 6).

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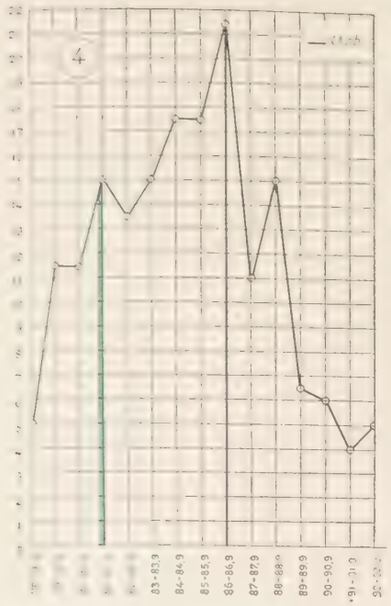
[1]) CHANTRE, Recherches anthropologiques dans l'Asie occidentale. Mémoires du Museum d'histoire Naturelle de Lyon T. 4, 1895.

[2]) MORANT, A preliminary classification of European races. Biometrika, Vol. **20** B. 1928.

[3]) KOHLBRUGGE, Systematisch en beschrijvend leerboek der Volkenkunde, 1930.

[4]) RIPLEY, Peoples of Europe, 1890, p. 363.

Table I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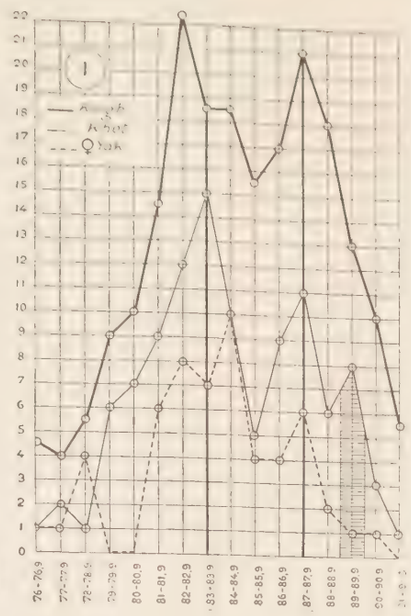
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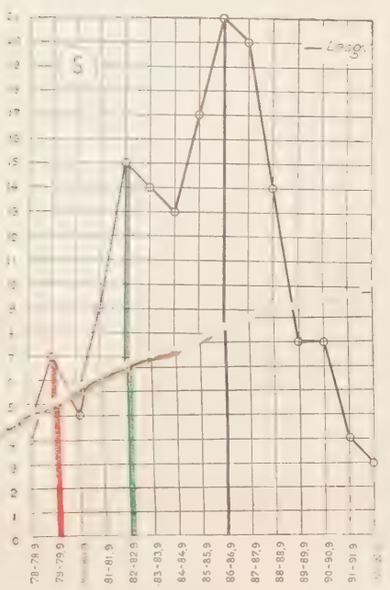
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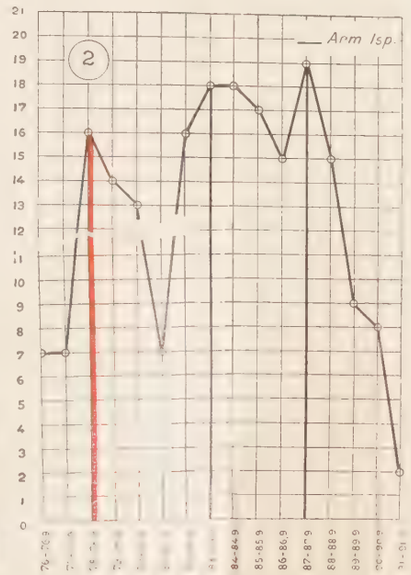
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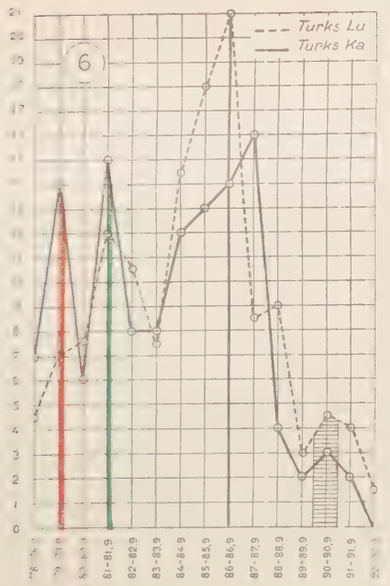
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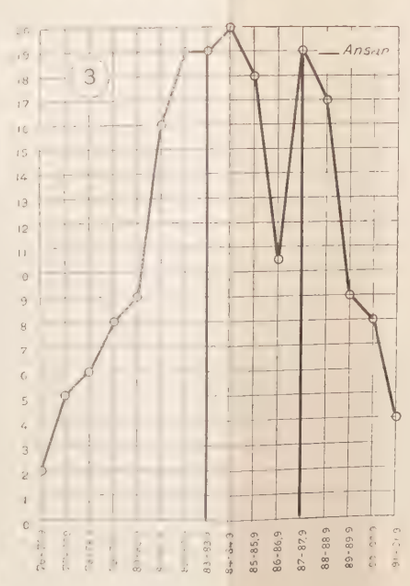
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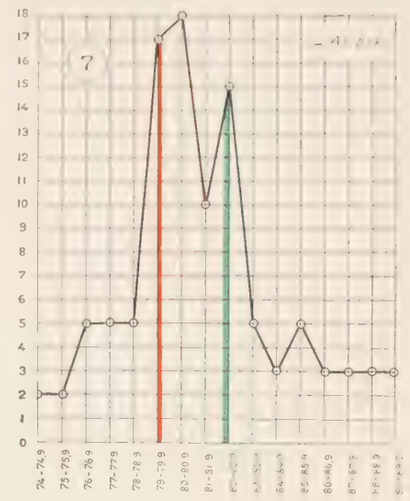
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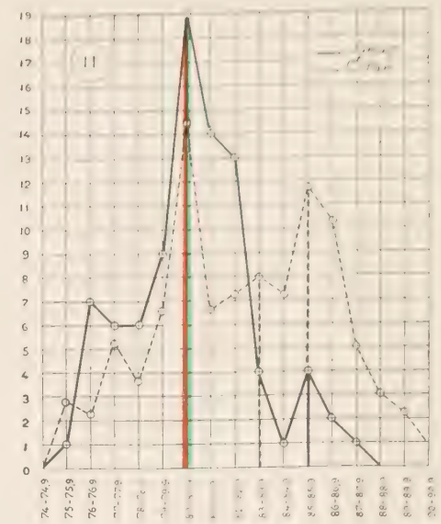
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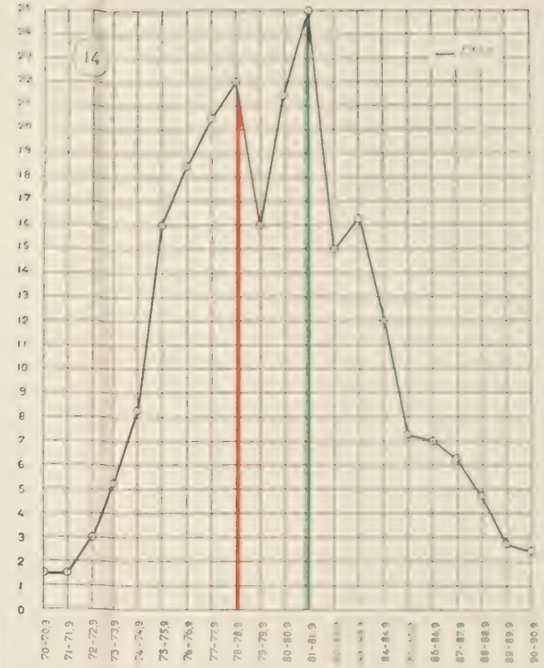
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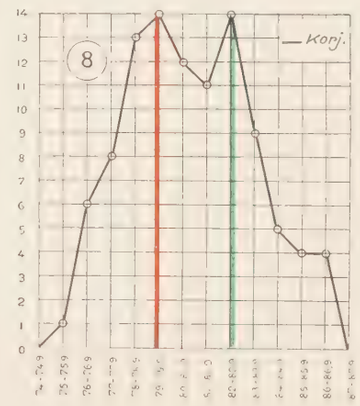
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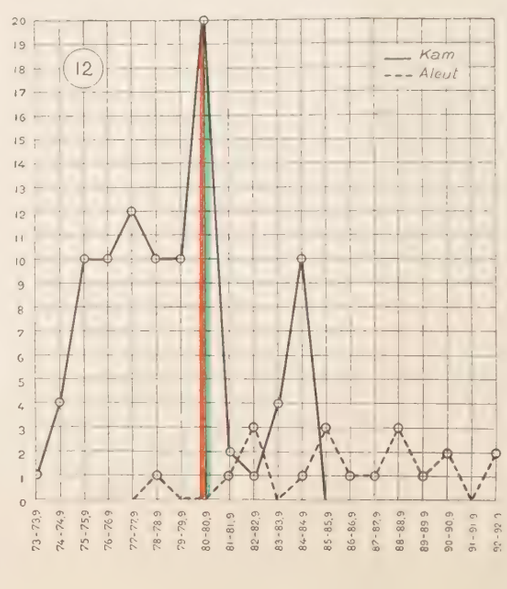
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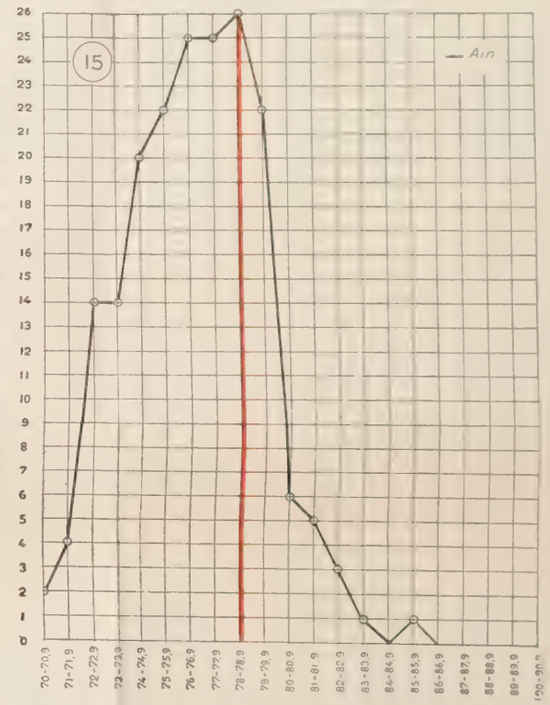
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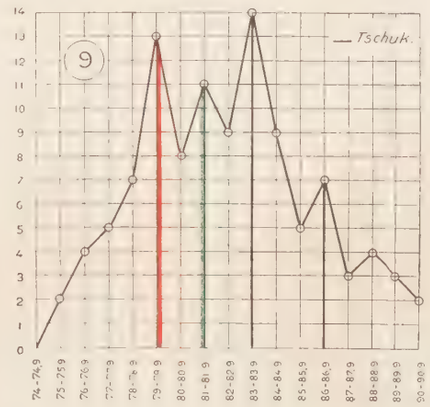
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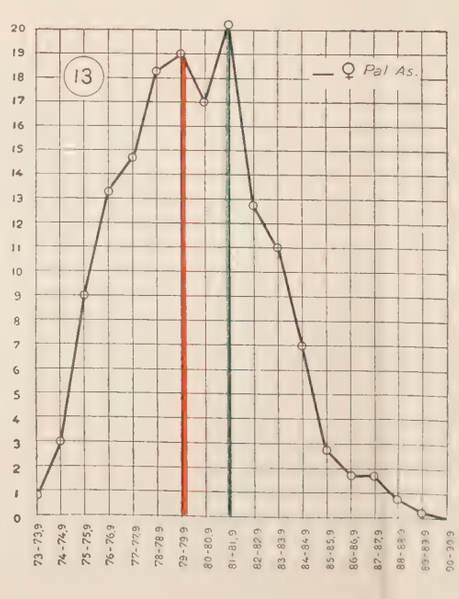
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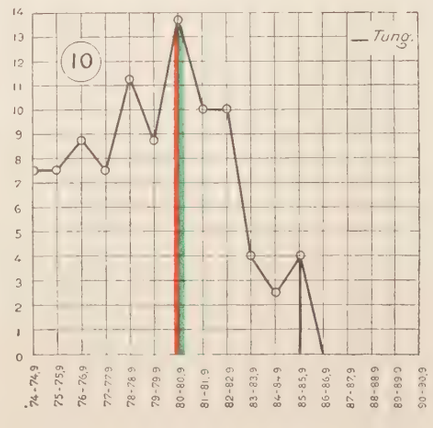
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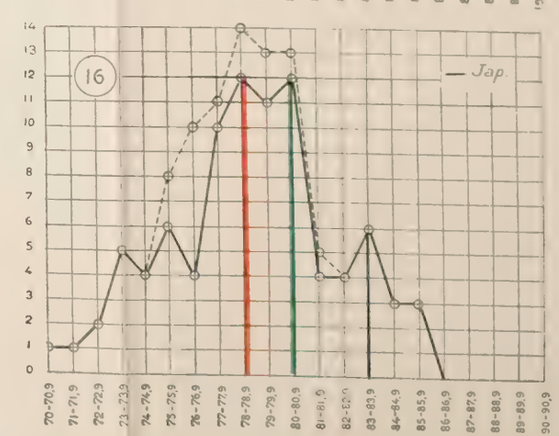
16
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With the Turks it is striking that their main peak (86) coincides with the main peak of VON ERCKERT's[1]) Lesghians (Table I, N°. 5) and OCHANINE's[2]) Uzbeks (N°. 4).

While, however, in these peoples the 83 (84) index, so often associated with the 86 (87) index, fails we find with the Turks and Lesghians an additional 79 and 81 (82) peak (with the Uzbeks an indication of an 82 peak).

These peaks, frequently associated, indicate an other racial influence, widely spread East as well as West to the Central Asiatic barrier but especially East, as appears from the index curves to the right of the black (barrier) line in Table I (Nos 7—16).

I shall return later to this category of associated indices, but before doing so shall deal with the Western spread of the Central Asiatic brachy- and hyperbrachycephalics, referring for this to Table II.

In the right top of Table II (N°. 1) I registered the male Jenessei Ostyaks recorded by ANUTCHINE[3]). Here again we have associated 83 and 85 peaks, occurring also among the Syrjans (Table II, N°. 2) and Ob-Samoyedes (83—87) measured by SOMMIER[4]) (N°. 3) and among DENIKER's Kalmucks[5]) (N°. 4: 83—87), in TSCHEPOURKOWSKY's Waldai type[6]) from Central and West Russia (N°. 6) and in the Germans from Pomerania between the Oder and Weichsel rivers, so thoroughly examined by REUTER[7]) (Table II, N°. 5).

In some of these people there are also indications of a (78) 79 or 81 peak or both. The 78 (79)- 81 (82) peaks, however, prevail in SOMMIER's Ob-Ostyaks (N°. 7), HELLA PÖCH's Volhynians (N°. 8; l.c. infra), in TSCHEPOURKOWSKY's Riasan type (Table II, N°. 10), and the Volga Tscheremisses recorded by SOMMIER[8]) (dotted curve in Table II, N°. 10).

Leaving the discussion of the latter peaks aside for the moment I first

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[1]) VON ERCKERT, Kopfmessungen Kaukasischer Völker. Arch. f. Anthropologie, Bnd. **18** and **19**, 1890 and 1891.

[2]) OCHANINE and JESCHEWICZ, Beiträge zur Anthropologie der Bevölkerung von Uzbekistan. Public. of the scientific department of the U.S.S.R. Commissariate Taschkend and Samarkand, 1929 (Russian).

[3]) ANUTCHINE, Anthropology of the Ostyaks (Russian) Moskow 1911. I am greatly indebted to Prof. V. STEFKO, Moskow for sending me a copy of this valuable paper. The Ob-Ostyaks registered by SOMMIER are very different, they apparently represent an other race. (cf. Table II N° 1 and N° 7).

[4]) SOMMIER, Arch. Antropologia e per l'Etnologia T. XVII.

[5]) DENIKER, Etude sur les Kalmouks, Revue d'anthrop. Serie II, T. VI et VII, 1883, 1884.

[6]) TSCHEPOURKOWSKY, Distribution géographique de la forme du crane etc. des paysans russes. Moskow, 1913. See also Arch. f. Anthrop., Bd. X, 1911.

[7]) F. REUTER, Beiträge zur Anthropologie Hinter-Pommerns. Arch. f. Anthropologie, Bnd. **28**, 1903.

[8]) SOMMIER, Note di viaggio. Esposizione Uralosiberiano di Ekateraburg. Ceremissi degli Urali e del Volga. Arch. per l'Antropologia e per l'Etnologia, Tomo **18**, 1888.

want to emphasize a feature occurring with the 83—83 groups, viz. the ultrabrachycephalic (89—90) index peak already indicated in Table I with the Kirghises (N⁰. 1) and Turks (N⁰. 6), and in Tables II, N⁰. 1 with the Jenessei Ostyaks and Ob-Samoyedes (N⁰. 3), but more evident in TSCHEPOURKOWSKY's Waldaitype (Table II, N⁰. 6), and with the Telengete Mongols of the Altai Tag (Table III, N⁰. 1) examined by REICHER [1]).

The curious fact with this ultrabrachycephalic peak is that it may and may not occur in groups of the same people, its appearance being apparently influenced by special conditions. So, while it fails with the 175 male Armenians I measured in Syria and Turkey (dotted line: Table III, N⁰. 2), it is very pronounced with the 200 female Armenians measured by Dr. KRISCHNER in Beirut (continuous line Table III, N⁰. 2). Yet its occurrence does not depend on sex, as appears from its presence among FRIZZI's [2]) Tirolese skulls, but not with TAPPEINER's [3]) Tirolese skulls (Table III, N⁰. 4), and from its occurrence among the male Kirghises and the male Turks.

Here we have to do with a greater change than merely the variation of one entity in the index peak, a change that points to a *mutationlike difference*.

It is important to note that this feature specially occurs with such groups of the 83 (84)—86 (87) index people in which the 86 (89) index prevails over the 83 (84) index.

So we find it with the Telengete skulls examined by REICHER (Table III, N⁰. 1), in KRISCHNER's female Armenians in which, contrary to my male Armenians (who do not show it) the 86 peak is higher than the 83 peak. Similarly with FRIZZI's Tirolese, in which it occurs, the 87 index group is higher than in TAPPEINER's Tirolese curve in which the ultrabrachycephalic peak fails (Table III, N⁰. 4). In the Waldai type (Table II, N⁰. 6), in which the 86 peak is higher than the 83 peak this ultrabrachycephalic peak again occurs. On the contrary it fails with RANKE's [4]) Bavarian skulls (Table III, N⁰. 5) in which the 83 (= 84 ceph. index) prevails over the 86 (=87 ceph. index), and with CWIRKO-GODYCKI's Bosnians (Table III, N⁰. 3) among whom the 86 peak is low.

The sole exceptions to this rule are DENIKER's Kalmucks (Table II, N⁰. 4), where the 86 peak is higher than the 83 one and yet an 89—90 peak fails, while in LIVI's Lombardians (Table III, N⁰. 6) and GRÜTZNER's girls from Zürich (Table III, N⁰. 9) where the 87 peak is low, an ultrabrachycephalic peak occurs.

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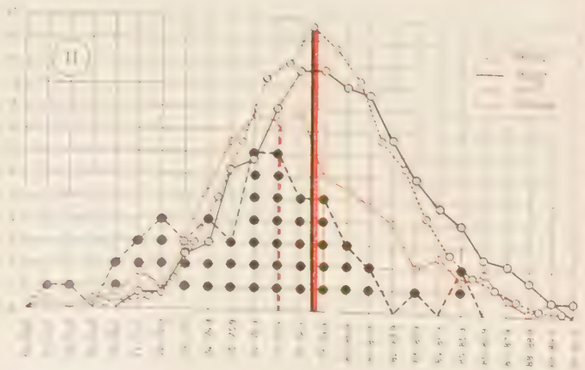
[1]) REICHER, Untersuchungen über die Schädelformen der Alpenländischen und Mongolischen Brachycephalen. Zeitschr. f. Morphol. u. Anthrop. Bnd. **15** and **16**, 1913.

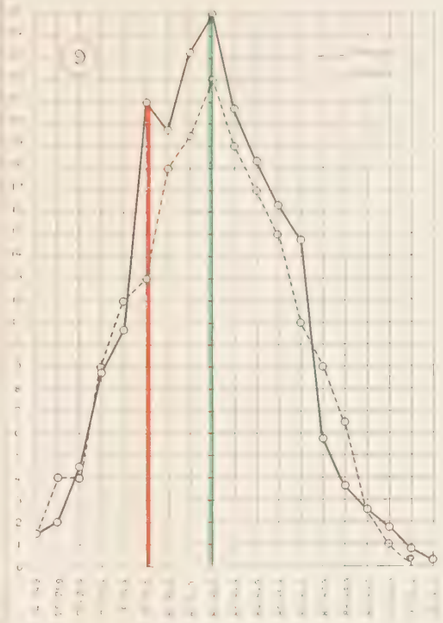
[2]) FRIZZI, Ein Beitrag zur Anthropologie des Homo Alpinus Tirolensis Mitt. der Anthrop. Gesellsch. in Wien. Bnd. **39** (N. F. Bnd. **9**) 1909.

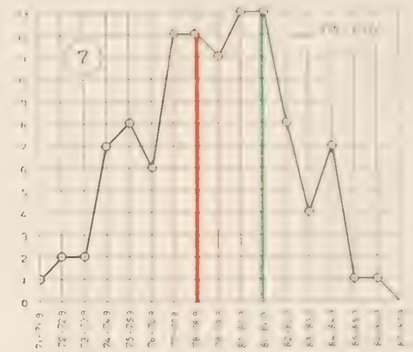
[3]) TAPPEINER, Beiträge zur Ethnologie und Anthropologie Tirols. Zeitschr. f. Ethnologie, Bnd. **12**, 1880.

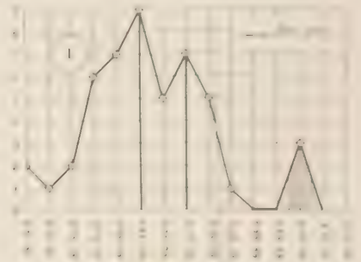
[4]) RANKE, Beiträge zur physischen Anthropologie der Baiern, 1883.

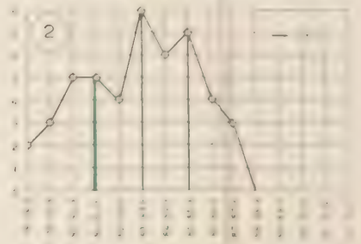
Table II

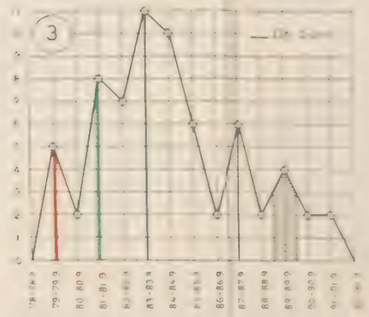


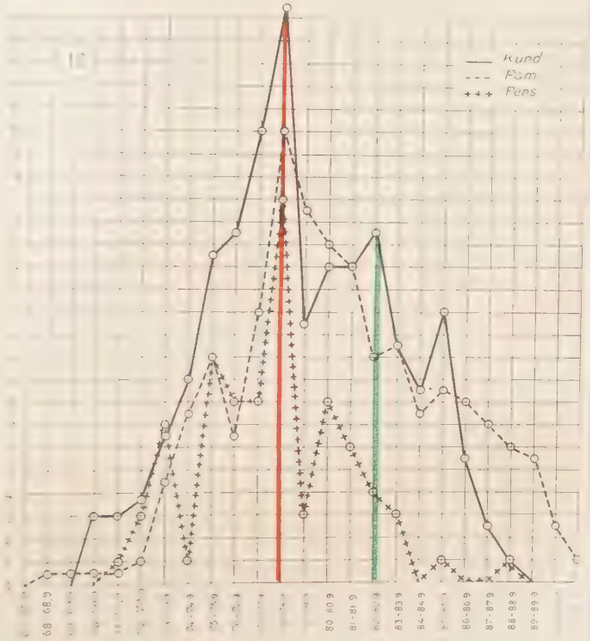


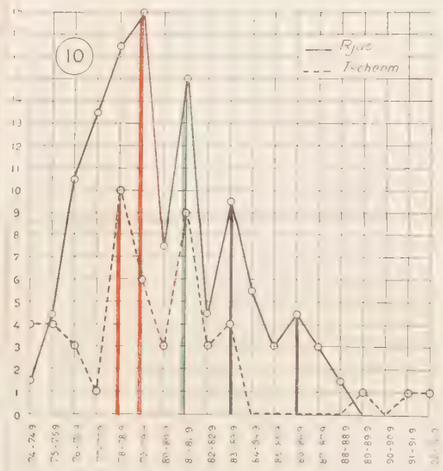


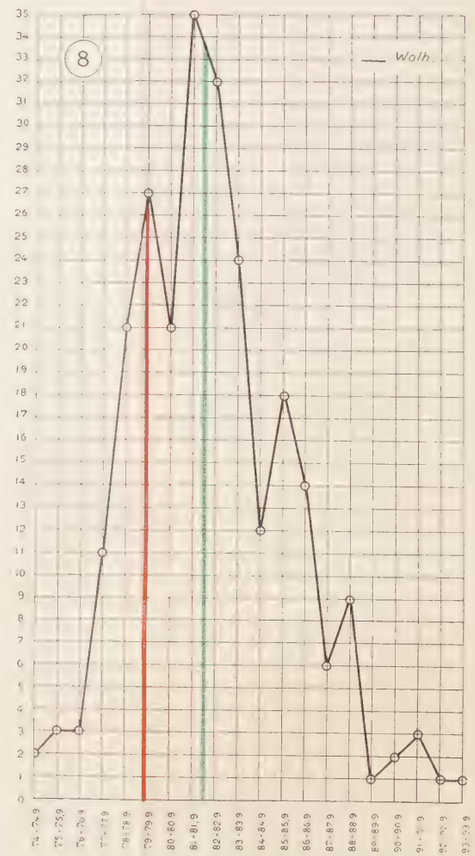


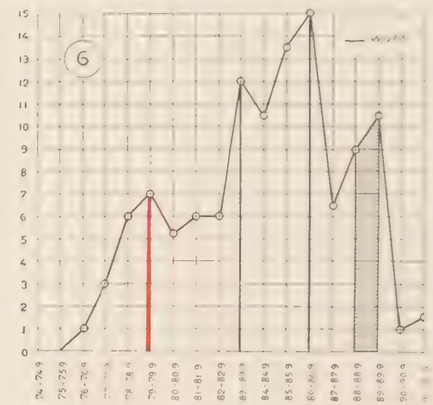


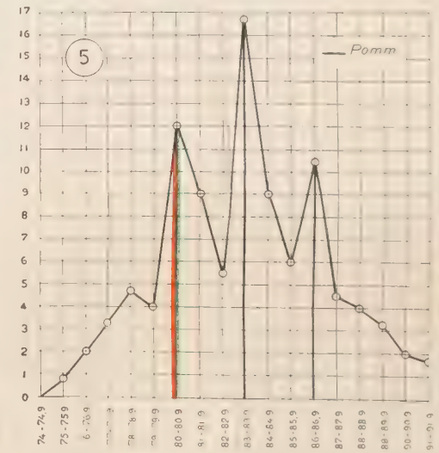


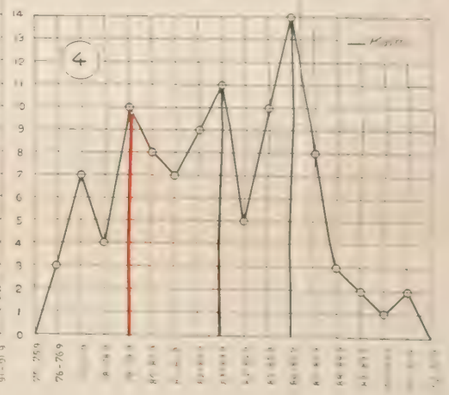












Commonly anthropologists, even those dealing with Asia, (DUDLEY BUXTON [1]), R. B. DIXON [2]), HADDON [3])) use the name Alpine race when speaking of these brachy-hyperbrachycephalic peoples. It is, however, evident that the expression central Asiatic brachycephals or — if one wants a name referring to its most typical European representatives — Dinaric race is better than Alpine.

Whereas the race in question was still very pure in the early middle ages in Alsace-Lorraine as appears from BLIND's [4]) measurements of 700 skulls (Table III, N°. 7) and is still represented — even with an additional 89 90 peak — in Auvergne (cf. the 172 skulls recorded by TOPINARD [5]): continuous line Table III, N°. 8), and may have occurred in addition to (an) other race(s) among the English round barrow people (dotted line Table III, N°. 8), Switzerland shows its representatives only in Graubünden in the so-called Dissentis type, less so in the North West. It is not strange that it occurs in this most Eastern corner of Switzerland since the influx of this people came from the East.

The type — as expressed in an index curve — is very characteristic with the 34 Dissentis skulls measured by RÜTIMEYER and HIS [6]), less so with the 41 Graubünden skulls measured by BLIND (l.c. supra). These skulls, grouped together in the two lower curves of Table III, N°. 10, even show an ultrabrachycephalic peak at 89, and this again is striking as the 86 and 87 skulls together are more numerous than the single 83 peak in this curve.

The large curve, however, constructed after WETTSTEIN's [7]) data concerning 252 skulls (Table III, N°. 10) can be accepted only with reserve as typical for our race considering the fact that the 84 cranial index peak corresponds with a 85 cephalic index and the cranial peak with a 88 cephalic one.

Nevertheless GRÜTZNER's [8]) measurements of 178 Swiss girls between 15 and 19 years from Zürich (Table III, N°. 9) show such characteristic 83 and 86 peaks that their relation to our race cannot be doubted, the less so as also here a small additional ultrabrachycephalic peak at 89 appears.

This curve, however, clearly shows that also the alpine regions of Switzerland contain a mixed population, there being in addition a very pronounced 78 and 80—81 peak among the indices of these girls.

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1) DUDLEY BUXTON, Peoples of Asia, ALFRED KNOP, New York, 1925.

2) R. B. DIXON, The racial history of man. SCRIBNER's Sons, New-York, London, 1923.

3) HADDON, Races of Man, Cambridge, University Press, 1924.

4) BLIND, Die Schädelformen der Elsässischen Bevölkerung in alter und neuer Zeit. Beitrage zur Anthropologie Elsas-Lotharingen's Heft I, Straszburg 1898.

5) TOPINARD, Eléments d'anthropologie générale.

6) RÜTIMEYER und HIS, Crania helvetica Baselu. Genf 1864.

7) WETTSTEIN, Zur Anthropologie und Ethnografie des Kreises Dissentis (Graubünden, Zürich 1902).

8) GRÜTZNER, Körperwachstum und Körperproportionen 15—19-jähriger Schweizerinnen, Zürich 1928.

Leaving aside for the moment the interpretation of these peaks and whether or not its 80 (81) peak corresponds with the 80 peak of the 175 Baden skulls recorded by MIES[1]) (Table III, N°. 10), I may refer to the fact that in the cephalic index curve of ZBINDEN's[2]) measurements of males in Luzern (and Simmenthal) a pronounced 79 and 82 peak occurs. It is, however, not my intention to deal with the anthropology of the Swiss. For this I may refer to SCHWERZ' interesting book[3]).

Index peaks at 79—81 (82) are typical for certain Russian groups as the Tscheremisses of the Volga (Table II, N°. 10: dotted line) and TSCHEPOURKOWSKY's Riasan type (the same figure, continuous line). In the males from Nowgorod (near Leningrad) the same peaks are still observed as appears from the measurements of 984 males, taken by TSCHEPOURKOWSKY and recorded in Table II, N°. 9 (continuous line). although here the 82 peak prevails, which in PARSON's[4]) 925 male Germans (dotted curve in the same figure) has became the only one.

This race is probably related to the additional groups occurring sporadically among the Syrjans, Samoyedes, Volga Kalmucks, Waldai Russians (Table II, N°. 2, 3, 4 and 6).

Most likely this (78) 79—81 (82) group is the Western representative of the Siberian races East to the Asiatic barrier: the Asiatic Eskimoes, Korjaks and Tschukschen. The curve of all the 463 Paleo-Asiatic women (Table I, N°. 13) measured by JOCHELSON-BRODSKY[5]) shows such a great resemblance with the female Russian Riasan type of TSCHEPOURKOWSKY (Table II, N°. 10 continuous curve) that a relationship can hardly be doubted. The same holds good for HELLA PÖCH's[6]) Volhynians (Table II, N°. 8).

Apparently the meso-subbrachycephalic Siberian races once extended along the whole Northern coast of Asia, descending East along the Pacific coast (cf. the Northern Chinese and Japanese curves Table I, N°. 14 and 16), and West between the Baltic and the Ural.

According to my index curves this race is widely spread in Russia and in the Caucasus and North Persia where among the Kurds and related Persians (Aderbeidjani a.o.) analogous relations are found (see for the Kurds the continuous line in Table II, N°. 12).

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1) MIES, Die Schädel der anatomischen Anstalt Heidelberg. Arch. f. Anthr. B. **24**, 1897.

2) ZBINDEN, Beiträge zur Anthropologie der Schweiz. Arch. f. Anthrop. B. **38** (N. F. 10) 1911. For these indexcurves see his paper.

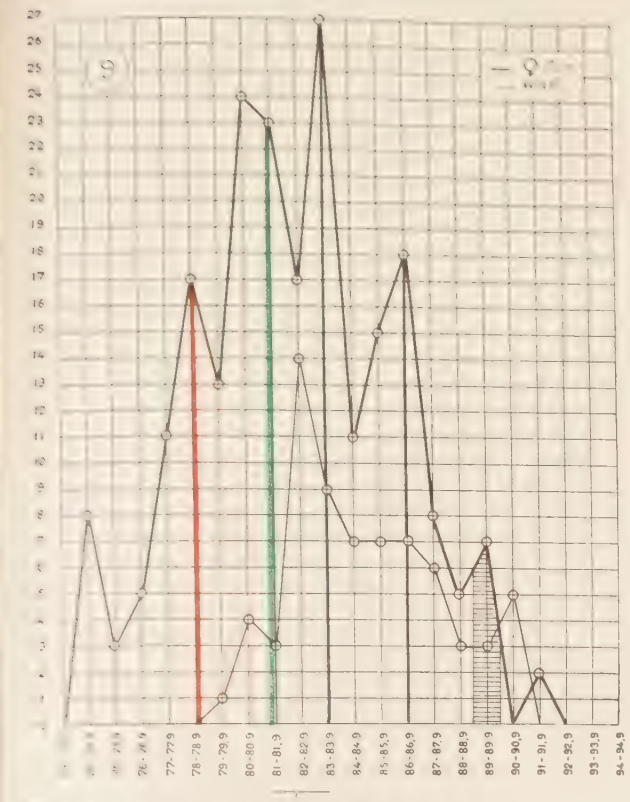
3) SCHWERZ, Die Völkerschaften der Schweiz von der Urzeit bis zur Gegenwart. STRECKER und SCHRÖDER, Stuttgart 1915, (see esp. p. 151).

4) PARSON's Anthropological Studies on German prisoners of war. Journ. of the Anthrop. Inst., Vol. **49**, 1919.

5) JOCHELSON-BRODSKY, Zur Topographie des weiblichen Körpers Nord- Ost Sibirischer Völker. Arch. f. Anthrop. Bnd. **33**, 1906. Making this curve I left out her female Yakuts, who are obviously different.

6) H. PÖCH, Mitt. der Anthr. Gesellsch. in Wien, Bnd. **45—46**, 1925—'26.

Table III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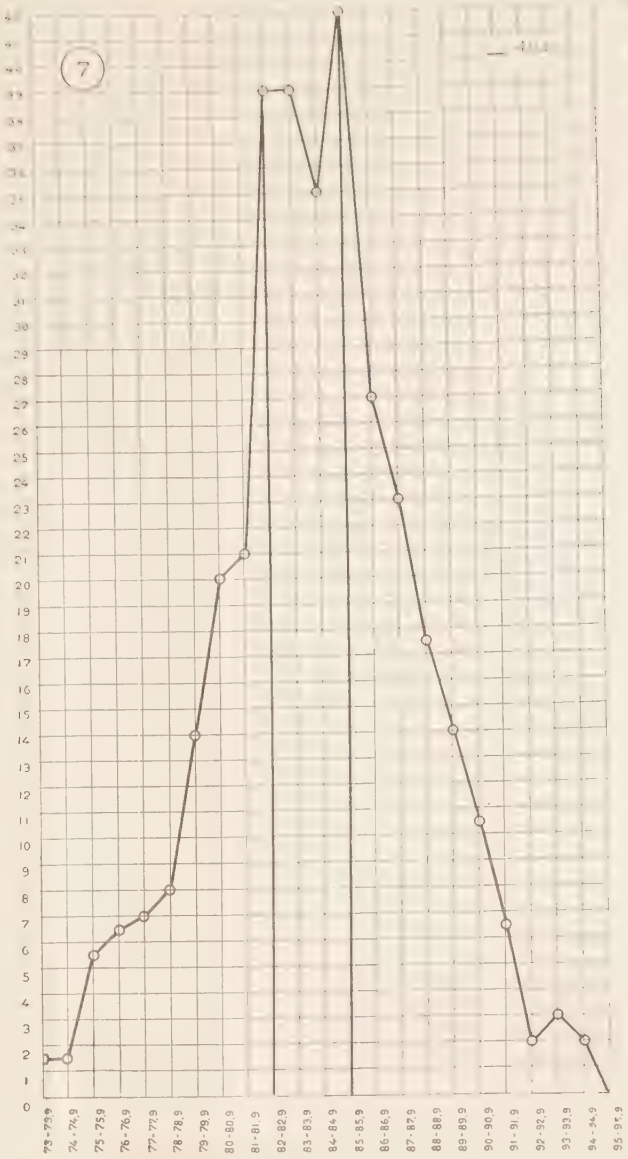
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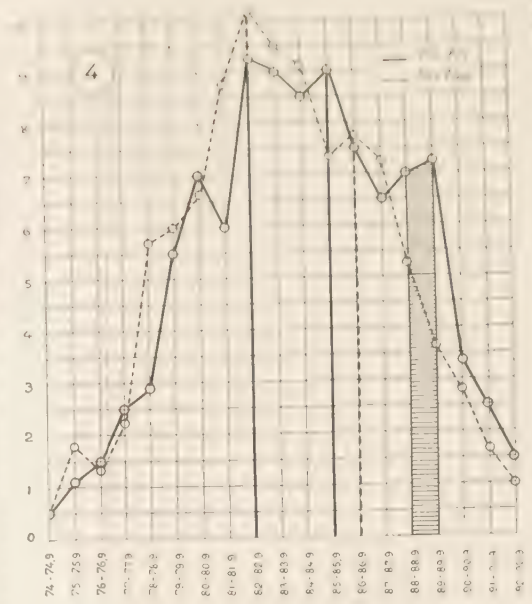
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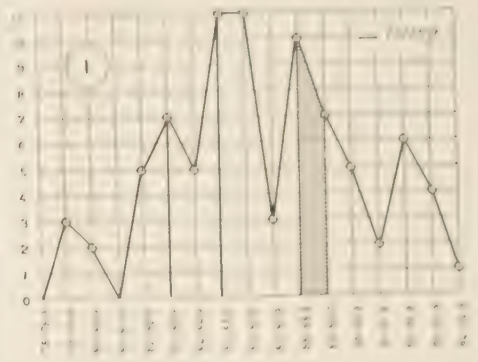
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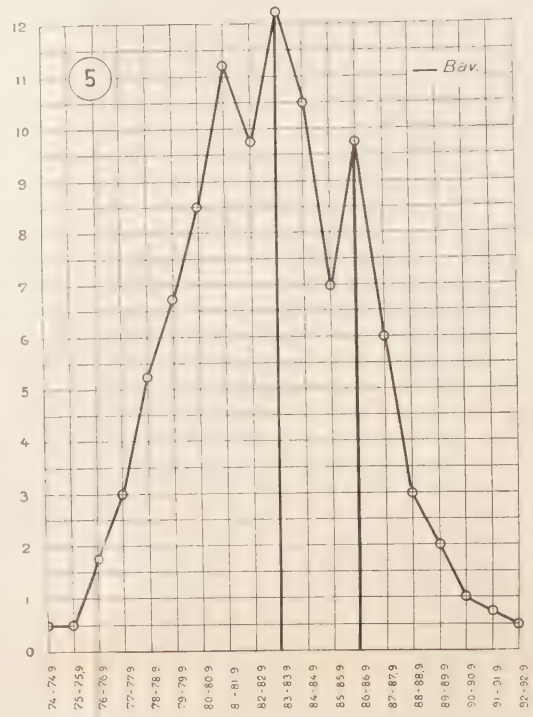
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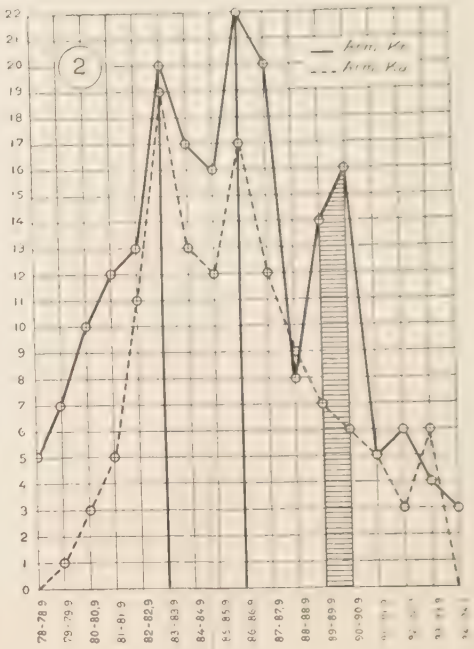
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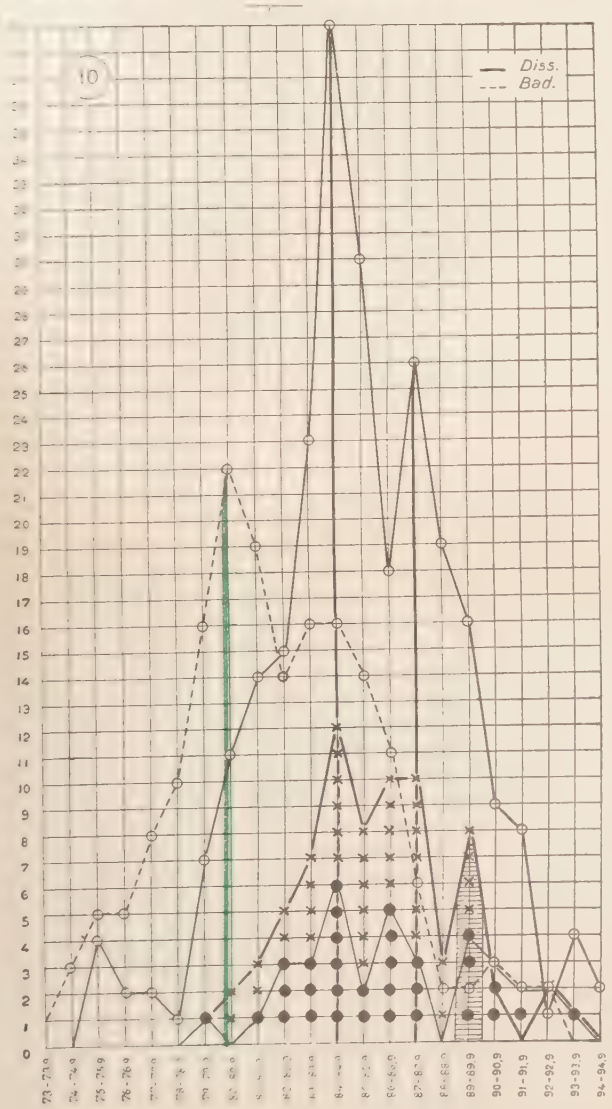
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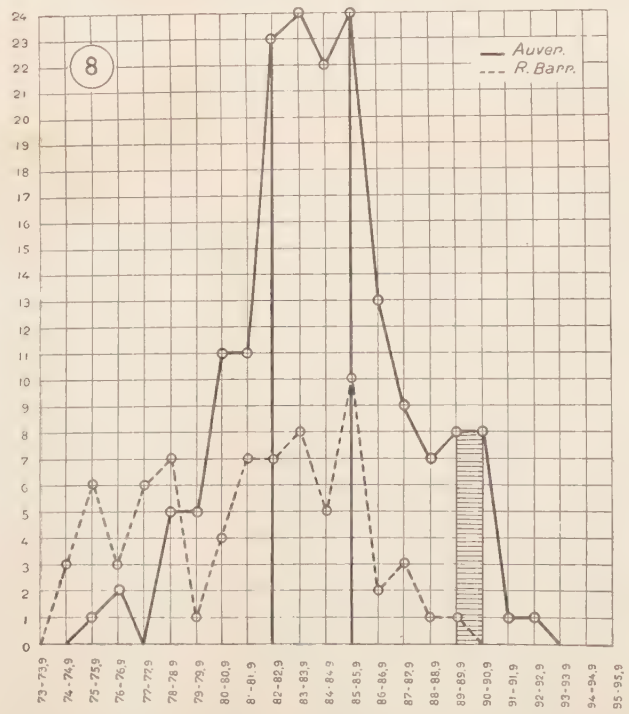
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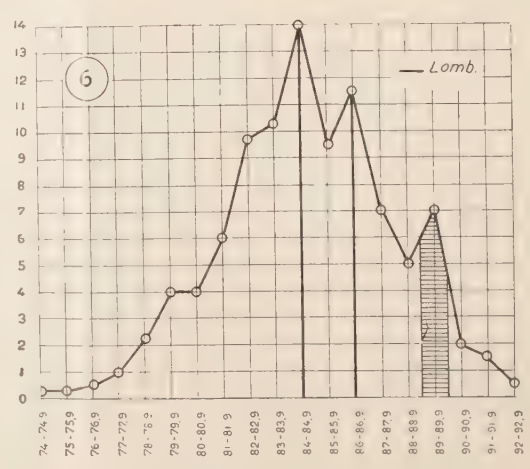
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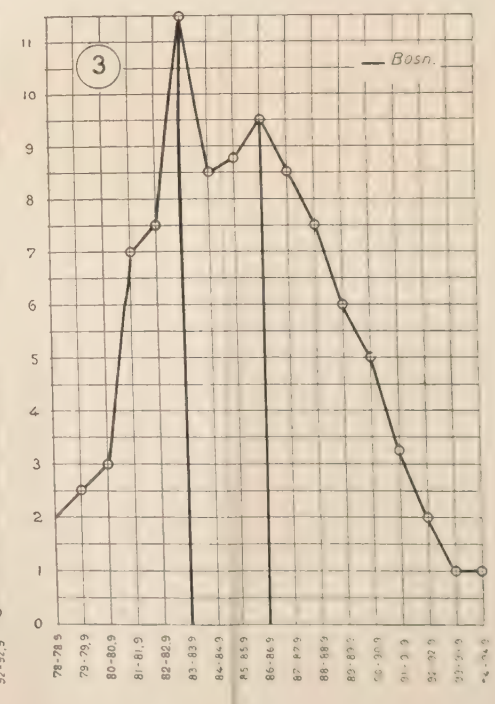
3
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The associated occurrence of the (78) 79—81 (82) peaks South of the Caucasus also explains their occurrence among the Mesopotamian Arabs and among the Palestinian Arabs and related people, recorded in former publications. The 79—82 peaks are also characteristic of the male Jews measured by WEISSENBERG [1]) in Cologne and Frankfort on the Maine.

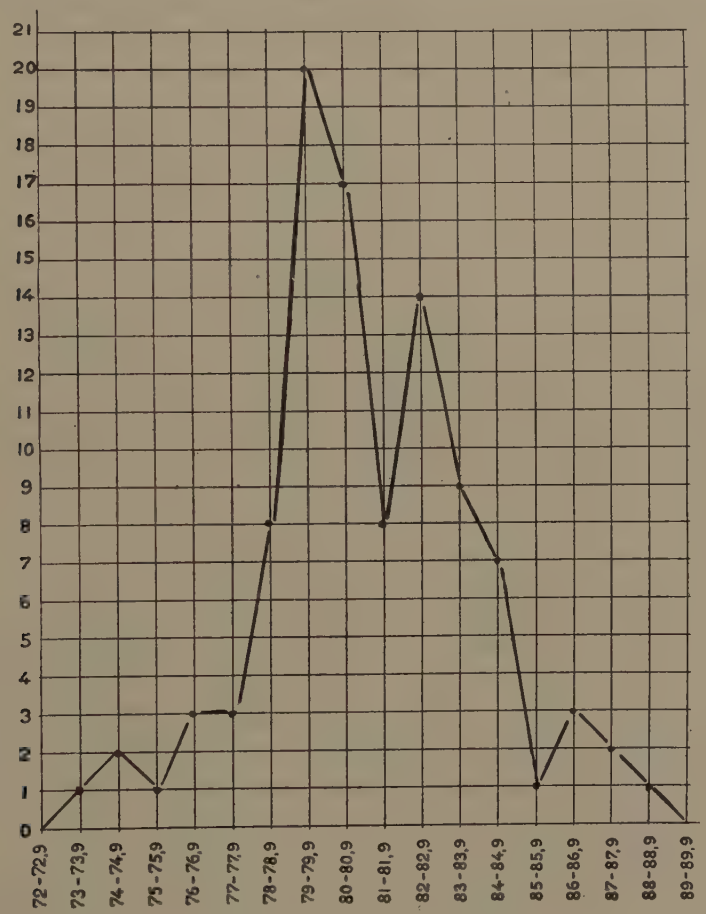

Fig. 2. Index curve of 100 male Jews measured by WEISSENBERG in Cologne and Frankfort a/M.

Similarly as we have to consider the 83 (84)—86 (87) cephalic index people as one large group occurring chiefly in Western Asia and Europe, we have to consider the (78) 79—81 (82) combination as a widely spread race. Both main races, however, the 83 (84)—86 (87) as well as the 78 (79)—81 (82) race, may show subdivisions and specialize in either one of their chief index peaks.

So we found that the 86 (87) index peak occurs isolated with the

1) WEISSENBERG, Zur Anthropologie der Deutschen Juden. Zeitschr. f. Ethnologie, Bnd. **44**, 1912. With the Amsterdam Askenasim I found a small peak at 78 and a very high one at 81.

Uzbeks, Lesghians, and Turks, while with the Yesidis and Mesopotamian Arabs the 83 group is by far the prevailing one[1]).

Similarly we find that the 78 (79)—81 (82) groups may crystallize in either a 78 (79) or a 81 (82) race.

This is seen in the East as well as in the West. In the East the Aino's (Table I N° 15) have a 76—78 cranial (78—79 cephalic) index, in the West the so-called Nordic, better called Norwegian race shows the same, as appears from the frequency curve of several thousands of Norwegians published by BRYN and SCHREINER[2]) (dotted line in Table II, fig. N°. 11).

Analogous relations obtain in England (PARSONS[3])) and in the Netherlands with the exception of Groningen and Zeeland; BOLK[4]). On the other hand among the Baltics the 82 index group has worked out, as appears from the Nowgorod curve. The same index prevails in the curve of 925 Germans based on PARSON's measurements (cf. Table II, N° 9).

The latter author, as well as R. B. DIXON (l.c.) and VON EICKSTEDT rightly state that the Nordic race, to which some German authors so frequently refer as being the main constituent of their people, nowadays forms a small constituent of this people (cf. also KEITER[5]), KLENCK and SCHEIDT[6])). It is prevailing only in Scandinavia, England and in the Netherlands in the provinces Holland and Friesland (BARGE[7])).

Yet far South an interesting group of the same race occurs viz. in Bulgaria and Rumania, as appears from the curve of 2000 Bulgars made by CWIRKO-GODYCKI[8]) (Table II, N°. 11, continuous curve).

As stated by J. L. MYRES[9]) the Vlachian Bulgars may be the representatives of the ancient Thrakians, who invaded pre-hellenic Hellas and in classic Greece established the majority of the population.

This Nordic type was represented also among the Scythians North of the Black Sea as it still occurs among the Iranian Persians.

That really this Nordic race may have acted a large part in the classic period of Greece appears from the small striped curve added in Table II.

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[1]) Perhaps the strong 82 peak in the index curve of skulls from the canton Wallis in Switzerland recorded by WACKER (Table III, N°. 9, dotted curve) has to be interpreted in the same way (see his paper zur Anthropologie der Walser etc., Zeitschr. f. Ethnol., Bnd. **44**, 1912).

[2]) BRYN and SCHREINER, Die Somatologie der Norweger nach Untersuchungen an Rekruten, Oslo 1929.

[3]) PARSONS, Anthropological observations on German prisoners of war. Journ. of the Anthrop. Institute, Vol. **49**, 1919.

[4]) BOLK, Kraniologische Untersuchungen an Holländische Schädel. Zeitschr. f. Morph. und Anthrop., Bnd. V, 1902.

[5]) KEITER, Schwansen und die Schlei. Schleswigsche Bauern und Fischer, Jena. Fischer, 1931.

[6]) KLENCK und SCHEIDT, Niedersächsische Bauern. Geestbauern im Elb.-Weser-Mündungsgebiet. Jena, Fischer, 1932.

[7]) BARGE, Friesche en Marker schedels. Dissertation Amsterdam, 1912.

[8]) CWIRKO-GODYCKI, Les Slaves méridionaux. Revue d'Anthropologie Année XL, '30.

[9]) J. L. MYRES, Who were the Greeks. California University Press 1930.

N°. 11, in which I registered all the Greek skulls of that period hitherto found. In comparing this curve with the cephalic Nordic curves in the same figure, we must realize that the cephalic index of mesocephals is one or two entities higher than the cranial one.

In order to provide a comparison with Nordic mesocephalic skulls I made a curve of the 300 skulls of the provinces Holland examined by BOLK (1 = 4 skulls) recorded in the same figure (red line). The parallelism between this curve and the Greek one is evident[1]).

Yet that the present Bulgarian group measured by CWIRKO-GODYCKI descends from the ancient Thrakians (Scytho-Iranians), who acted such a large part in the structure of the classic Greek population is not so sure, since we may remember that 1000 years after the golden age of Greece other Nordic settlers, the Goths, invaded these regions.

Whatever may be the case with these Bulgars, so much is sure that also on the Northern shores of the Black sea, the Caucasus and Persia (the plateau of Iran) in ancient as well as in recent times the 78—79 index, either combined (Kurds) or not (Bulgars and Aderbeidjani) with a 81 (82) index peak was very frequent and this Irano-Scythian people (even still numerous in the Pamirs) certainly belongs to the same groups of mesocephalics, who had such an influence on the population of Greece and Crete already in late Helladic and late Minoan, but especially in Hellenic times.

Before ending I still want to remind that in the "Introduction to the Anthropology of the Near East in ancient and recent times", attention was called to what seems to be a mutability of the index in dolichocephalic races, in the opposite sense as found with brachycephalics, since with 71 cranial index peak races as the Indo-Aryans from Central Europe examined by SCHLIZ and RECHE, the Panjabi examined by HAVELOCK CHARLES (l.c. fig. 1) and the ancient Minoan (l.c. fig. 69) as well as modern Sardinian skulls (l.c. fig. 87) examined by DUCKWORTH, in addition to an outstanding 71 index peak an 67 index peak occurs. I do not consider this peak as an indication of an admixture with another race but rather as a mutation based perhaps on a sort of leptosome constitution, that may be caused by unfavourable conditions. It fails with the Caspian Indo-Aryans examined by myself (l.c. fig. 1).

Résumé.

Resuming my results I may state that in Asia North of the Himalaya, as well as in Europe North of the Mediterranean especially two races occur. One, the brachy-hyperbrachycephalic race has spread chiefly West to the Central barrier.

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1) I apologize that I used a Netherland series of skulls for such a proud comparison but it was the only mesocephalic Nordic skull series at my disposal. I am convinced that BRYN and SCHREINER could provide us with a much larger one and refer also to the cephalic index peak of English males published by PARSONS (l.c.).

In some cases the brachycephalic, in others the hyper-brachycephalic element may be the most or even the only outstanding index in this race. Furthermore those representatives of this race in which the hyper-brachycephalic 86 (87) peak prevails show a tendency to have an additional ultra-brachycephalic (89—90) peak. The occurrence of this ultra-brachycephalic peak points to a mutation that raises the question if the 83 and 86 peaks may also be mutations of each other, in this sense that in their ancestral groups both possibilities: the formation of a prevailing 83 or a prevailing 86 (87) peak occurred, the appearance of which may have depended on especial conditions: environment, food, endocrine factors, or combinations of these three factors.

East as well as West to the Central barrier the paleo-asiatic 78 (79)—81 (82) index peak race extends. Also this race may specialize either in a Nordic 78 (79) peak group or in a Baltic 81 (82) group.

As in all the cases recorded the mutability range of the index curves is about three units in the frequency peaks, this reminds us the mutation range, observed among some dolichocephalic Indo-Aryan (and Mediterranean) groups in which the prevailing 71 skull index peak is frequently complicated by a hyper-dolichocephalic (67) peak.

EXPLANATION OF CURVES [1].

TABLE I.

No. 1. Thin continuous line: male ceph. ind. in Khotan and Turfan (AUREL STEIN. JOYCE).
Thick continuous line: the same plus the ceph. indices of 37 Kirghises, measured by the same, and 242 Kirghises measured by DE UFFALVY (1 = 2).
Dotted line: 56 female Yakuts measured by Mrs. JOCHELSON BRODSKY.

No. 2. Cephalic indices of 176 male Armenians from New-Julfa near Isphahan, measured by KRISCHNER.

No. 3. Cephalic indices of 193 Alouites or Ansarieh, 145 measured by KAPPERS, 48 by CHANTRE.

No. 4. Ceph. ind. of 390 male Uzbeks measured by OCHANINE (1 = 2).

No. 5. Ceph. ind. of 163 male Lesghians measured by VON ERCKERT.

No. 6. Dotted line: ceph. index of 796 male Turks measured by VON LUSCHAN; continuous line: 138 Turks measured by KAPPERS.

No. 7. Ceph. index of 60 male Asiatic Eskimoes recorded by JOCHELSON BRODSKY; expressed in percentages.

No. 8. Ceph. ind. of 169 male Koryaks, recorded by JOCHELSON BRODSKY; expressed in percentages.

No. 9. Ceph. ind. of 148 male Tschukschen, recorded by JOCHELSON BRODSKY; expressed in percentages.

No. 10. Ceph. ind. of 76 male Tunguses, recorded by JOCHELSON BRODSKY; expressed in percentages.

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[1]) Wherever there is a difference between the number of cases mentioned in this list and the number registered in my curves this is due to the fact that — to save space — I have cut off the extremes of the spread of the curve if only few indices, not characteristic of the group, were represented in these extremes.

No. 11. Continuous line: ceph. ind. of 59 male Jakughirs, recorded by Mrs. JOCHELSON BRODSKY; expressed in percentages.
Dotted line: ceph. ind. of 137 male Yakuts, measured by HECKER.
No. 12. Continuous line: ceph. ind. of 63 male Kamtschadales, recorded by Mrs. JOCHELSON BRODSKY; expressed in percentages.
Dotted line: cranial index of 18 Aleutian skulls measured by BESSELS (15), and MONTANDON (3).
No. 13. Ceph. index of all the female N. E. Asiatics (463) measured by JOCHELSON BRODSKY (1 = 3) with the exception of her female Yakuts (registered in Table I No. 1, dotted curve).
No. 14. Ceph. ind. of 942 male Chinese, chiefly from North China (Kiangsi, Shantung, Chihli) and Manchuria; measured by KOGANEI (1 = 4).
No. 15. Cranial ind. of 191 Aino skulls, measured by KOGANEI, KOPERNICKI and others.
No. 16. Continuous line: cranial indices of 88 Japanese skulls, measured by ADACHI.
Dotted line: the same plus 10 skulls measured by TOLD, and 4 measured by BLIND.

TABLE II.

No. 1. Ceph. ind. of 48 male Jenessei-Ostyaks, measured by ANUTCHINE.
No. 2. Ceph. ind. of 49 Syrjans (25 males and 24 females), measured by SOMMIER.
No. 3. Ceph. ind. of 67 male Ob-Samoyedes, measured by SOMMIER.
No. 4. Cranial (or cephal.?) indices of 104 Kalmucks recorded by DENIKER.
No. 5. Cephalic ind. of 189 Pomeranian young men, measured by REUTER.
No. 6. Cephalic index curve of female Waldai Russians recorded by TSCHEPOURKOWSKY; expressed in percentages.
No. 7. Ceph. ind. of 109 Ob-Ostyaks, measured by SOMMIER.
No. 8. Ceph. ind. of 246 male Volhynians from the Ukraine, measured by HELLA PÖCH.
No. 9. Continuous line: ceph. ind. of 984 males from the gouvernment Nowgorod, measured by TSCHEPOURKOWSKY (1 = 5);
dotted line: ceph. ind. of 925 male Germans measured by PARSONS (1 = 5).
No. 10. Continuous line: Ceph. ind. of the female Rjasan Russians, measured by TSCHEPOURKOWSKY; expressed in percentages.
Dotted line: ceph. index of 50 male and female Tscheremisses from the Volga, measured by SOMMIER.
No. 11. Dotted line: Ceph. index curve of several thousands of Norwegian males measured by BRYN and SCHREINER, expressed in percentages;
continuous line: ceph. ind. of 2000 Bulgars measured by CWIRKO-GODYCKI; expressed in percentages.
Red line: cranial index of 300 skulls from the province Holland, measured by BOLK (1 = 4).
Striped line: cranial index of 53 Hellenic skulls (dots) of the classic period, measured by various authors.
No. 12. Continuous line: cephalic index of 378 male Kurds (272 measured by CHANTRE, 106 by KAPPERS).
Dotted line: ceph. ind. of the largely mesocephalic groups of the Pamirs recorded by Sir AUREL STEIN and JOYCE. (354 males).
Crossed curve: ceph. ind. of 81 female Persians from Isphahan measured by Mrs. M. KRISCHNER.

TABLE III.

No. 1. Cranial ind. of 82 Altai Telengete skulls, measured by REICHER.

No. 2. Continuous line: ceph. ind. of 200 female Armenians, measured in Beirut by KRISCHNER.
Dotted line: ceph. ind. of 136 chiefly male Armenians, measured by KAPPERS in Beirut and Constantinople.
No. 3. Ceph. ind. of 4752 male Bosnians, measured by CWIRKO-GODYCKI; expressed in percentages.
No. 4. Continuous line: cranial indices of 1124 Tirolese skulls measured by FRIZZI.
Dotted line: cranial ind. of 1319 Tirolese skulls measured by TAPPEINER.
Both expressed in percentages.
No. 5. Cranial ind. of 1000 Bavarian skulls, measured by RANKE; expressed in percentages.
No. 6. Ceph. ind. of 36202 male Lombardians, measured by LIVI; expressed in percentages.
No. 7. Cranial indices of 700 mediaeval skulls from the Alsace, measured by BLIND.
No. 8. Continuous line: cranial ind. of 172 Auvergnate skulls, recorded by TOPINARD.
Dotted line: cranial index of 74 English Round Barrow skulls, recorded by the same.
No. 9. Continuous: Ceph. ind. of 214 girls from 15—19 years, measured at Zürich by GRÜTZNER.
Dotted line: cranial index of 69 skulls from Vorarlberg (Wallis), measured by WACKER.
No. 10. Lower (thin) continuous line (dots): cranial index of 33 Dissentis skulls, measured by RÜTIMEYER and HIS. Second (thicker) continuous line: the same plus the cranial ind. of 41 Graubünden skulls, measured by BLIND (crosses).
High continuous line: cranial indices of 252 Dissentis skulls measured by WETTSTEIN.
Dotted line: cranial indices of 175 Baden skulls, measured by MIES.

Physics. – *Measurements about the velocity of sound in neon gas.* By W. H. KEESOM and J. A. VAN LAMMEREN. Abstract of Communication No. 234c from the Kamerlingh Onnes Laboratory.

(Communicated at the meeting of October 27, 1934).

The velocity of sound in neon gas has been measured from 0° C down to liquid neon temperatures. Therefor a new vapour cryostat for obtaining the last mentioned temperatures was designed.

From the measurements down to liquid oxygen temperatures, making use of values of B (the second virial coefficient) deduced by CATH and KAMERLINGH ONNES, values of C_p/C_v for $p=0$ were calculated. The results agree within 0.2 % with the expected value 1.667.

At the temperatures 27.80 and 26.25° K the dependence of the velocity of sound on pressure was investigated.

From the results in combination with CATH and KAMERLINGH ONNES' values of B the following values were calculated:

T (°K)	30	40	50	60	70	80
$-10^3\ B$	4.40	2.48	1.55	1.02	0.68	0.46

Values of atomic heats at constant pressure and at constant volume for the temperatures 27.80 and 26.25° K and pressures from 0 to 1.0 or 0.6 atm respectively were calculated.

Physics. — *On the separation of neon into its isotopic components by means of rectification.* By W. H. KEESOM, H. VAN DIJK and J. HAANTJES. Abstract of Communication No. 234*d* from the Kamerlingh Onnes Laboratory.

(Communicated at the meeting of October 27, 1934).

A rectifying apparatus with 60 pans, entirely made of glass, has been constructed for the purpose of rectification of liquid neon in order to separate the isotopes of this element in relatively large quantities. It consists principally of the column proper of about 2 m length, an outer Dewar glass of the same length and above these glasses another vacuum vessel of special construction, to contain the cooling liquid, in this case liquid hydrogen.

In September 1933 a rectification was made which lasted 4 days. 420 l neongas was liquefied in the column. The rectification was divided into three periods. In the first period the liquid was rectified for 18 hours without taking off any gas. At the end of this time a sample (I) of 4 l was taken from the condensation space. In the second period of 64 hours a quantity of 280 l neon gas was regularly taken off from the condensation space. In the third period of 10 hours rectification was again carried out without taking off anything. At the end of this last period a sample (II) of 5 l was taken from the still. 300 l liquid hydrogen were used in the cooling. The gas density determinations gave as results:

Sample I	$S_{0_I} = 0.00089580$	atomic weight	20.091
,, II	$S_{0_{II}} = 0.00091733$	,, ,,	20.574
	$S_{0_n} = 0.00089990$	,, ,,	20.183

S_0 is the density at standard conditions, S_{0_n} is the density of normal neon.

If we compare our results with theory it appears that the efficiency of the column is about 50 % of the theoretical value. The better results than those formerly obtained in our rectifying experiments may be attributed to the following: The great number of pans, a large reflux ratio and a great regularity of the rectification process.

Mathematics. — *Zur Theorie der Kombinanten.* Von R. WEITZENBÖCK.

(Communicated at the meeting of October 27, 1934).

Die ganzen rationalen Kombinanten eines Formenbüschels $\lambda_1 f + \lambda_2 g$ kann man definieren als Polynome der projektiven Komitanten der Basisformen f und g, die bei der dreigliedrigen einfachen Gruppe $\mathfrak{G}_3$ der linearen Büscheltransformationen invariant bleiben. Baut man $\mathfrak{G}_3$ aus den infinitesimalen Transformationen auf, so ergibt sich die Möglichkeit, die Kombinanten schrittweise zu konstruieren, vorausgesetzt, dass das volle System der Formen f und g bekannt ist. Die allgemeine Methode hierzu habe ich in einer früheren Arbeit dargestellt[1]). Es soll hier an einem einfachen Beispiele gezeigt werden wie diese Methode arbeitet, wobei das Resultat natürlich nicht neu und durch die GORDAN'sche Methode schneller zu erhalten ist[2]).

§ 1.

Zwei binäre quadratische Formen

$$f = a_x^2 = a_{11} x_1^2 + 2a_{12} x_1 x_2 + a_{22} x_2^2 \quad , \quad g = \alpha_x^2 = \alpha_{11} x_1^2 + 2\alpha_{12} x_1 x_2 + \alpha_{22} x_2^2$$

haben als volles Komitantensystem neben f und g selbst noch die quadratische Kovariante

$$\varphi = (a\alpha)\, a_x\, \alpha_x$$

und die drei Invarianten

$$A_{11} = (ab)^2 \quad , \quad A_{12} = (a\alpha)^2 \quad , \quad A_{22} = (\alpha\beta)^2.$$

Eine Kombinante von f und g ist ein Polynom der sechs soeben aufgezählten Formen, das bei den binären Transformationen

$$(T) \ldots \begin{cases} \bar{f} = \lambda_1 f + \lambda_2 g \\ \bar{g} = \mu_1 f + \mu_2 g \end{cases} \qquad (\lambda_1 \mu_2 - \lambda_2 \mu_1 = 1)$$

die Invarianteneigenschaft hat. Alle T bilden eine dreigliedrige einfache Gruppe $\mathfrak{G}_3$, die erzeugt werden kann durch die drei infinitesimalen Transformationen

$$C(F) = \frac{\partial F}{\partial \lambda_1}\lambda_1 - \frac{\partial F}{\partial \lambda_2}\lambda_2 \quad , \quad A(F) = \frac{\partial F}{\partial \lambda_1}\lambda_2 \quad , \quad B(F) = \frac{\partial F}{\partial \lambda_2}\lambda_1 ;$$

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[1]) Acta mathem. **58** (1933), p. 267, § 15.

[2]) P. GORDAN, Mathem. Ann. **5** (1872), p. 95.

in den Koeffizienten von f und g geschrieben lauten sie

$$C(U)=\sum_{ik}\left(\frac{\partial U}{\partial a_{ik}}a_{ik}-\frac{\partial U}{\partial \alpha_{ik}}\alpha_{ik}\right),\quad A(U)=\sum_{ik}\frac{\partial U}{\partial a_{ik}}\alpha_{ik},\quad B(U)=\sum\frac{\partial U}{\partial \alpha_{ik}}a_{ik}.$$

Die Kombinanten sind dann die $\mathfrak{G}_3$-Invarianten der sechs Argumente

$$(U_\nu)=f,\ g,\ \varphi,\ A_{11},\ A_{12},\ A_{22} \quad . \ . \ . \ . \ . \ . \ . \quad (1)$$

und ihre Ermittlung erfordert nach der allgemeinen Theorie die Ausführung folgender Schritte:

1. Bestimmung der C-Invarianten J_C der Veränderlichen U_ν.
2. Berechnung von $A(J_C)$ und Ermittlung aller linear-unabhängigen Basisformen V_ν dieser $A(J_C)$.
3. Berechnung der A-Invarianten J_A der Veränderlichen V_ν.
4. Ermittlung der absoluten A-Invarianten W_ν in den Veränderlichen V_ν und schliesslich
5. Bestimmung der absoluten C-Invarianten K der Veränderlichen W_ν.

Diese K sind dann die gesuchten Kombinanten.

Wie die Gleichungen

$$\left.\begin{aligned} C(f)=f\ ,\ C(g)=-g\ ,\ C(\varphi)=0\ ,\\ C(A_{11})=2A_{11}\ ,\ C(A_{12})=0\ ,\ C(A_{22})=-2A_{22}\end{aligned}\right\} \quad . \ . \ . \quad (2)$$

zeigen, sind die U_ν bereits C-Invarianten. Der zweite Schritt gilt der Berechnung der $A(J_C)=A(U_\nu)$ und wird dargestellt durch die Tabelle

	f	g	φ	A_{11}	A_{12}	A_{22}
f	0	1				
g		0				
φ			0			
A_{11}				0	2	
A_{12}					0	1
A_{22}						0

D.h. es ist

$$A(f)=g\ ,\ A(A_{11})=2A_{12}\ ,\ A(A_{12})=A_{22}$$

und alle anderen $A(U_\nu)$ sind Null.

Die V_ν fallen hier also mit den U_ν von (1) zusammen.

Der dritte Schritt: φ und A_{22} sind absolute A-Invarianten. Die übrigen ergeben sich aus der Tabelle entsprechend der 1 im ersten Feld und der Reihe 2, 1 im dritten Feld als Invarianten einer binären Linearform und einer binären quadratischen Form:

$$D=A_{11}A_{22}-A_{12}^2 \qquad \Phi=A_{11}g^2-2A_{12}fg+A_{22}f^2.$$

Hier ist Φ wegen der Syzygie $\Phi=-2\varphi^2$ reduzibel und D ist eine absolute Invariante, sodass hier der vierte Schritt wegfällt und die absoluten A-Invarianten W_ν gegeben werden durch

$$(W_\nu)=g,\ \varphi,\ A_{22},\ D.$$

Ein Potenzprodukt $P = g^{p_1} A_{22}^{p_2} D^{p_3}$ gibt bei C nach (2) und wegen $C(D) = 0$

$$C(P) = (-p_1 - 2p_2) \,.\, P;$$

bei absoluten C-Invarianten muss also $-p_1 - 2p_2 = 0$ sein, d.h. $p_1 = p_2 = 0$. Der fünfte und letzte Schritt liefert demnach als Integritätsbasis für die absoluten C-Invarianten K:

$$(K) = \varphi,\ D,$$

womit auch die Basis für die Komitanten ermittelt ist.

Chemistry — *On Complex Salts of α-α'-Dipyridyl with Bivalent Copper.* II. By F. M. JAEGER and J. A. VAN DIJK.

(Communicated at the meeting of October 27, 1934).

§ 1. In previous papers on complex salts of α-α'-dipyridyl and bivalent metals [1]) we have drawn attention to the fact that the number of molecules of the base introduced into the complex ion evidently is often dependent on the special nature of the anion of the metallic compounds used in the reaction. Especially in the case of the ferrous salts — sulphates and chlorides — and in that of the cupric chloride and sulphate this fact was stated beyond all doubt. As a further proof of the correctness of this view, we here communicate the results obtained in the reaction between α-α'-dipyridyl and copper *nitrate*.

If to a solution of copper nitrate in water an alcoholic solution of 1, 2 or 3 equivalents of dipyridyl is added, the colour of the solutions turns in all three cases to a deep blue. On slow evaporation, dark blue crystals of the *tri-* and *di-*dipyridyl-salts are soon deposited: in contrast to what was observed of the cupric salts previously described, the solution of the *mono-*dipyridyl salt does not easily crystallize and proves to contain the most soluble compound of the three; finally, however, also from this solution a dark blue salt is deposited, which is a true *mono-*dipyridyl-cupric nitrate. Cupric nitrate, therefore, readily forms complex salts with dipyridyl, corresponding to all three types mentioned.

§ 2. *Mono-dipyridyl-diaquo-cupric nitrate* has the composition:

$$\left\{ Cu \begin{matrix} (Dipyr) \\ (H_2O)_2 \end{matrix} \right\} (NO_3)_2 + 1\ H_2O.$$

The salt crystallizes in aggregates of thick tabular crystals or in separate, smaller, needle-shaped crystals of a dark blue colour; they are distinctly dichroïtic. Analysis: 16.04 % Cu; 13.38 % H_2O; theoretically: 15.98 % Cu and 13.59 % H_2O. Evidently the salt has a composition analogous

[1]) F. M. JAEGER and J. A. VAN DIJK, Proceed. R. Acad. Sciences Amsterdam, **37** (1934), 10, 333; 395.

to the following, — one molecule dipyridyl in it being substituted by two molecules of water.

The crystals are triclinic-pinacoidal; their axial ratio is: $a:b:c = 1.4411:1:0.7766$, with:

$$A = 91°\,23'; \quad \alpha = 101°\,56'$$
$$B = 105\ 51; \quad \beta = 109\ 42$$
$$C = 58\ 2; \quad \gamma = 56\ 8$$

Forms observed: $a = \{100\}$, predominant and lustrous; $b = \{010\}$, very lustrous and well developed; $c = \{001\}$, mostly rather broad and lustrous, sometimes, however, very narrow; $r = \{101\}$, broad and very lustrous; $m = \{110\}$, well developed and yielding good reflections, but less brilliant than a or c; $\omega = \{\bar{1}\bar{1}1\}$, small but well reflecting, with more or less oscillating angular values; $s = \{\bar{1}01\}$, extremely narrow, only approximately measurable, often absent. The habitus is elongated parallel to the b-axis and either tabular or needle-shaped. (Fig. 1).

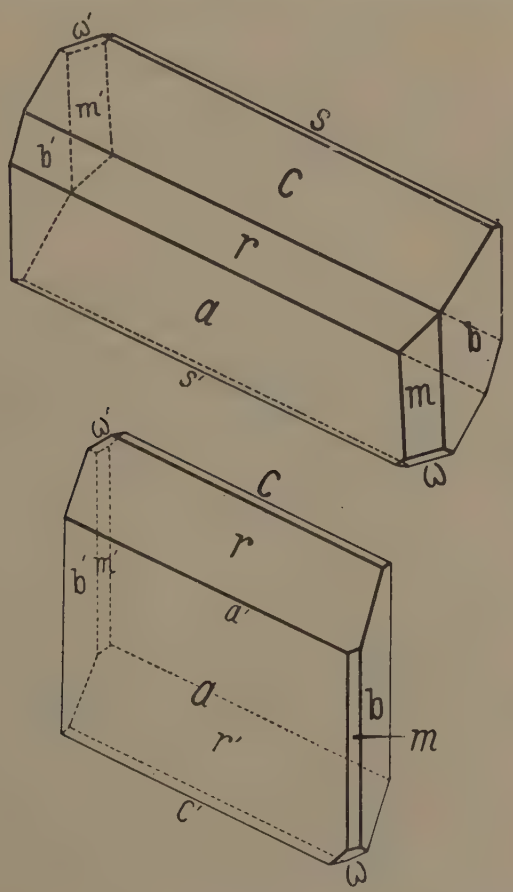

Fig. 1. Crystalform of Monodipyridyl-Cupric-Nitrate (+ H_2O)

Angular Values:		*Observed*		*Calculated:*	
$a : c$	$= (100) : (001) =^{*}$	74°	9′	—	
$a : b'$	$= (100) : (0\bar{1}0) =^{*}$	58	2	—	
$c : b'$	$= (001) : (0\bar{1}0) =^{*}$	91	23	—	
$a : m$	$= (100) : (110) =^{*}$	77	16	—	
$a : r$	$= (100) : (10\bar{1}) =^{*}$	46	39	—	
$m : b$	$= (110) : (010) =$	44	42	44°	42′
$r : c$	$= (10\bar{1}) : (001) =$	27	30	27	30
$c : m$	$= (001) : (110) =$	75	14	75	17
$a' : \omega'$	$= (\bar{1}00) : (\bar{1}\bar{1}1) =$	95	30	95	28
$b' : r$	$= (0\bar{1}0) : (10\bar{1}) =$	76	18	76	20
$\omega : c$	$= (\bar{1}\bar{1}1) : (001) =$	42	18	42	37
$a' : s$	$= (\bar{1}00) : (\bar{1}0\bar{1}) =$	*ca*	69°	69	33
$c : s$	$= (001) : (\bar{1}0\bar{1}) =$	*ca*	37°	36	28
$b' : \omega$	$= (0\bar{1}0) : (\bar{1}\bar{1}1) =$	*ca*	61°.5	61	36
$m : \omega'$	$= (110) : (11\bar{1}) =$	*ca*	61°.5	62	6

No distinct cleavability was observed. On $\{100\}$ oblique extinction, at about 44° with the edge $a : r$. Dichroïtic; on $\{100\}$, for vibrations in a direction including 45° with $a : r$, dark blue; for such perpendicular to those, pale blue.

§ 3. *Didipyridyl-Cupric-nitrate* has the composition: $\{Cu\ (Dipyr)_2\}(NO_3)_2 + 1\ H_2O$; the analysis gave: Cu: 12.21 %; H_2O: 3.37 %; calculated: 12.27 % Cu and 3.48 % H_2O. The salt is less soluble than the previous one. From water it crystallizes at room-temperature in thick, not very perfectly built crystals. They are triclinic, but represent pseudo-monoclinic limiting forms.

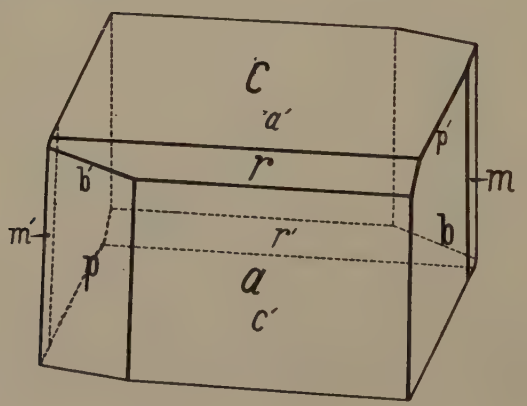

Fig. 2. Crystalform of Didipyridyl-Cupric-Nitrate (+1 H_2O)

Triclinic-pinacoidal (pseudo-monoclinic)

$a : b : c = 2.0562 : 1 : 1.3385;$

$A = 90° 38'$; $\alpha = 90° 13'$

$B = 111\ 52$; $\beta = 111\ 51^1/_3$

$C = 91\ 9$; $\gamma = 90\ 59$

Forms observed: $a = \{100\}$ and $c = \{001\}$, both very well developed, very lustrous, but often yielding multiple reflections; $b = \{010\}$. broad and very lustrous; $r = \{101\}$, well developed and giving good images; $m = \{1\bar{1}0\}$, very lustrous, mostly narrow; sometimes a curved, rough face $\{10.\bar{1}.0\}$ occurs as a vicinal form and also a very small vicinal form $\{11.0.1\}$. The habitus is short or thickly prismatic, with an elongation parallel to the *b*-axis. (Fig. 2).

Angular Values:	*Observed*:	*Calculated*:
$a : c = (100) : (001) =$	$*\ 68°\ 8'$	—
$a : b = (100) : (010) =$	$*\ 88\ 51$	—
$b : c = (010) : (001) =$	$*\ 89\ 22$	—
$a : m = (100) : (1\bar{1}0) =$	$*\ 63\ 15$	—
$a : r = (100) : (101) =$	$*\ 42\ 12$	—
$m : r = (1\bar{1}0) : (101) =$	$70\ 46$	$70°\ 40'$
$b : r = (010) : (101) =$	$88\ 49$	$89\ 0$
$m : b' = (1\bar{1}0) : (0\bar{1}0) =$	$27\ 54$	$27\ 44$
$c : r = (001) : (101) =$	$25\ 56$	$25\ 56$

No distinct cleavability was observed.

On $\{001\}$ for vibrations almost perpendicular to the edge $a : c$, the colour is dark blue, for such at right angles with those, greenish blue; this dichroïsm is only weak. The extinction on $\{100\}$ and $\{001\}$ is oblique, under a very small angle (4° or 5°) with respect to the edge $a : c$.

§ 4. *Tridipyridyl-Cupric Nitrate*: $\{Cu\,(Dipyr)_3\}(NO_3)_2 + 6\,H_2O$ is deposited from its aqueous solution in the shape of hexagonally bordered, flat, dark blue crystals. They are very plastic, because of the presence of gliding planes; their edges are often rounded and their angular values are rather variable. Analysis yielded: Cu: 8,33 %; H_2O: 14,10 %; N: 15,09 %, theoretically: 8,33 % Cu; 14,15 % H_2O; 14,7 % N.

Monoclinic-prismatic.

$a : b : c = 1{,}6577 : 1 : 1{,}4960.$

$\beta = 87° 0'.$

Forms observed: $c = \{001\}$, predominant, yielding very sharp images; $a = \{100\}$, dull, mostly horizontally striated; $\omega = \{111\}$ and $o = \{\bar{1}11\}$,

almost equally broad and giving good reflections; $m = \{110\}$, only small and narrow, moderately reflecting. The habitus is tabular parallel to

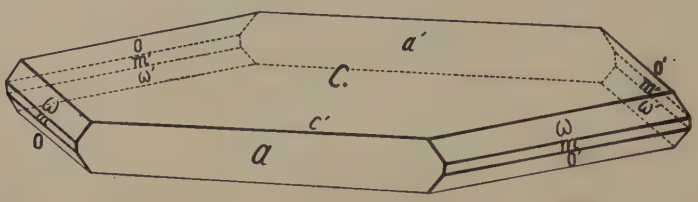

Fig. 3. Crystalform of Tridipyridyl-Cupric-Nitrate (+6H₂O)

$\{001\}$, with a slight elongation in the direction of the *b*-axis. The salt is less soluble than the *di*-dipyridyl salt just described.

Angular Values:	*Observed:*		*Calculated:*	
$a : c = (100) : (001) =$	* 87°	0′	—	
$c : o = (001) : (\bar{1}11) =$	* 62	50	—	
$o : o = (\bar{1}11) : (11\bar{1}) =$	* 80	45	—	
$c : \omega = (001) : (111) =$	60	45	60°	36′
$o : \omega = (\bar{1}11) : (111) =$	50	40	50	$12^1/_2$
$\omega : m = (111) : (110) =$	27	43	27	51
$o : m = (11\bar{1}) : (110) =$	28	51	28	43
$a' : o = (\bar{1}00) : (\bar{1}11) =$	66	4	66	$23^1/_2$
$a : \omega = (100) : (111) =$	63	16	63	24

Cleavable parallel to $\{001\}$; this plane is also a gliding plane. The crystals are only feebly dichroïtic: on $\{001\}$ blue and pale blue. On $\{001\}$ normal extinction; the plane of the optical axes is perpendicular to $\{010\}$; the first bissectrix is almost perpendicular to $\{001\}$. The apparent axial angle is only small.

In thin layers, this salt has a red colour; it often appears in the final fractions of the crystallisation, but the crystalform and the composition are the same as here indicated.

In the case of copper nitrate, as well as in that of copper chloride[1]), therefore, all three types of salts are possibly generated, in contrast to the case of copper *sulphate*, where only a *mono-* and a *tri-*dipyridyl derivative could be obtained.

§ 5. *Acetates.* From a solution of 1 mol. *copper acetate* with 1, 2 or 3 equivalents of dipyridyl a *mono-dipyridyl-copper acetate* of the composition: $\left\{ Cu \begin{matrix} (Dipyr) \\ (H_2O)_2 \end{matrix} \right\} (C_2H_3O_2)_2 + 3H_2O$ is obtained. Analysis yielded: 14,73 %

[1]) F. M. JAEGER and J. A. VAN DIJK, these Proceed., **37**, (1934) 395.

Cu; 6.49 % N and 20.61 % H_2O; theoretically: 14.86 % Cu, 6.55 % N and 21.06 % H_2O. The crystals have a rather dark blue colour with a reddish surface-hue; the prismatic needles are clearly dichroïtic: dark blue for vibrations parallel to their elongation, dark blue-violet for those perpendicular to this direction. They show an oblique extinction. The flat prisms are very badly built; in the vertical zone some angles were measured: b (broad and lustrous): m (narrow and not very lustrous) = 29° 19′; $m : p$ (like m, striated) = 31° 4′; $p : a$ (narrow, but well reflecting) = 30° 34′; $a : t$ (broader, well reflecting) = 31° 39′; $t : b'$ = 57° 24′. Most probably the crystals are triclinic.

From the solution of copperacetate with 2 equivalents of dipyridyl, when slowly evaporated at 15°, an aggregation of blue crystals was obtained. As still the odour of the dipyridyl was perceptible, the crystals, after drying, were treated with ethylacetate. Analysis then yielded the following results: 11.68 % Cu; 18.84 % H_2O and 48.0 % dipyridyl, which corresponds to a *di*-dipyridyl-derivative, in which $^1/_3$ molecule of the base is replaced by $^2/_3$ mol. of water and containing 5 H_2O as water of crystallisation: $\left\{ Cu \begin{matrix} (Dipyr)_{1.66} \\ (H_2O)_{0.66} \end{matrix} \right\} (C_2H_3O_2)_2 + 5\ H_2O$. Of course, it also were possible, that here a mixture were present of 1 mol. of the salt formerly described: $\left\{ Cu \begin{matrix} (Dipyr) \\ (H_2O)_2 \end{matrix} \right\} (C_2H_3O_2)_2 + 3\ H_2O$ and of 2 mol. $\{ Cu\ (Dipyr)_2 \{ (C_2H_3O_2)_2 + 6\ H_2O$. Circumstances seem to be somewhat analogous as observed in the case of the salts of ironsulphate formerly investigated.

A pure salt with 2 mol. dipyridyl within its complex ion, never was obtained. A solution of copperacetate + 3 equivalents of dipyridyl gave a blue salt, yielding: 9.97 % Cu; 18.79 % H_2O and 52.4 % dipyridyl; as it still smells of dipyridyl, the crystals again were treated with ethylacetate: now they proved to contain: 8.51 % N, 14.00 % Cu and 12.53 % H_2O. This composition corresponds to a salt of the formula: $\left\{ Cu \begin{matrix} (Dipyr)_{1.38} \\ (H_2O)_{1.24} \end{matrix} \right\} (C_2H_3O_2)_2 + 2\ H_2O$. Evidently, the pure *di*-dipyridyl-salt cannot be obtained in this way; it always proves to be hydrolized to some extent.

Groningen, Laboratory for Inorganic and Physical Chemistry of the University.

Chemistry. — *Les Potentiels d'écoulement du Sulfate de Baryum.* Par H. R. KRUYT et R. RUYSSEN.

(Communicated at the meeting of October 27, 1934).

Les travaux de P. F. J. A. JULIEN [1]) sur les potentiels électrocinétiques du bromure d'argent ont montré que les solutions de nitrate d'argent ne permettaient pas un renversement du signe négatif initial du potentiel d'écoulement d'un capillaire en bromure d'argent massif. Il est avéré cependant que le bromure d'argent colloïdal migre vers la cathode en présence de nitrate d'argent même en concentration relativement faible [2]). Ces travaux ont abouti à la conclusion que seuls les cristaux très fins, et non ceux à gros grains, pouvaient subir une inversion du signe de leur charge; d'où découlait l'hypothèse que les ions Ag^+ étaient fixée uniquement aux arêtes des cristaux [3]). Ces vues étaient d'ailleurs confirmées par les essais d'orientation que JULIEN avait étendus à d'autres substances, et où il avait fait usage de la méthode cataphorétique. Il était très indiqué d'appliquer ces mêmes mesures de potentiels d'écoulement à une autre substance, sel insoluble, et d'emblée notre choix tomba sur le sulfate de baryum.

Il est certain que les potentiels de contact, à la surface des cristaux sont dominés par des phénomènes d'adsorption. L'adsorption d'électrolytes aux cristaux de sulfate de baryum a fait l'objet de recherches étendues [4]), qui nous permettaient, mieux que pour n'importe quelle substance, d'approfondir la question.

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[1]) Thèse Utrecht, 1933.

[2]) A. LOTTERMOSER, Journ. f. Prakt. Chem. (2) **72**, 39, 1905.
K. FAJANS, Trans. Far. Soc. **16**, A 103, 1920.
H. R. KRUYT et P. C. VAN DER WILLIGEN, Zeitschr. f. Phys. Chem. (A) 139?, **53**, 1928.
E. J. W. VERWEY et H. R. KRUYT, Zeitschr. f. Phys. Chem. A **167**, 137, 1933; et ibid. p. 149; et ibid. p. 312.

[3]) Cette hypothèse a été émise également pour d'autres phénomènes (cfr. A. SMEKAL, Z. Elektroch. **35**, 567, 1929; H. S. TAYLOR, Proc. Roy. Soc. London (A) **108**, 105, 1925; J. Physic. Chem. **30**, 145, 1926.

[4]) L. DE BROUCKERE, Thèse d'agrégation; Bruxelles 1932 (avec la bibliographie complète des travaux).
TEZAK, Koll. Zeitschr. **59**, 158, 1932.

Mode expérimental. [1])

Les appareils de mesure étaient les mêmes que ceux déjà décrits antérieurement [2]): l'écoulement dans les capillaires se faisait par une pression d'azote réglée par un manostat à $^1/_{10}{}^e$ de mm de Hg; les potentiels étaient mesurés par la lampe électromètre PHILIPS 4060. Les batteries étaient en tous points semblables à celles décrites. Vu le point de fusion très élevé du sulfate de baryum nous n'avons pas envisagé la possibilité d'obtenir des capillaires par fusion suivant la méthode décrite par JULIEN [3]). Les mesures ont donc été faites par la méthode au diaphragme; elle consistait à faire couler la solution aqueuse le long des cristaux de sulfate de baryum serrés dans un tube en verre Iéna (ouverture 4 mm) entre deux tampons de toile de gaze en platine. Nous avons vainement essayé une méthode semblable à celle de BRIGGS [4]), qui nous aurait permis de mesurer la résistance spécifique en place. Cette méthode ne se prêtait pas à un montage serré, qui seul évitait l'entraînement des microcristaux. Dès que le sulfate de baryum était déplacé ou entraîné à travers le diaphragme, les potentiels ne restaient plus constants et variaient à chaque inversion du sens d'écoulement.

Nous avons apporté les plus grands soins à la préparation des diaphragmes d'une résistance d'écoulement appropriée (environ 0.5 cc/minute sous une pression de 36.25 cms Hg) et contenant chaque fois une quantité de sulfate de baryum sensiblement la même. Les différents diaphragmes accusaient cependant un certain écart pour les valeurs des ζ (calculées suivant la formule de HELMHOLTZ—SMOLUCHOWSKI). Ils étaient par exemple pour une solution de 100 micromoles de $BaCl_2$ au litre: $+$ 10.2, $+$ 11.8, $+$ 9.8, $+$ 12.3, $+$ 11.8 mvolts.

Pour chaque courbe (potentiels/concentrations) les mesures étaient faites le plus souvent avec le même diaphragme. Au besoin les valeurs des diaphragmes différents étaient comparées en mesurant les potentiels d'écoulement avec une même solution de référence de 100 μ moles de $BaCl_2$ au litre et corrigées dans le même rapport. Ceci n'a d'ailleurs du être fait que pour deux mesures. (Tableau V 100 μ $ZnCl_2$ et tableau II 50 μ $BaCl_2$).

Nous avons vérifié que pour nos capillaires la fonction E/P restait constante dans la limite des pressions de mesure (applicabilité de la loi de POISEUILLE: voir valeurs du tableau I où $E =$ force électromotrice en mvolts et $P =$ pression en cms Hg). [5]).

Afin de connaître l'influence de la grosseur des grains, les potentiels furent mesurés à différents échantillons de sulfate de baryum.

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1) Nous ne communiquons ici que les résultats des expériences.

2) H. R. KRUYT et R. RUYSSEN, Proc. Ac. Sciences Amsterdam **37**, 498 (1934).

3) Loco Cito. p. 15.

4) D. BRIGGS, J. Phys. Chem., **32**, 641, 1928; ibid. p. 1646.

5) Toutes les mesures de potentiel ultérieures ont d'ailleurs été faites à une même pression de 36.25 cms de Hg.

TABLEAU I.

Sulfate de Ba précipité (II)				Baryte naturelle	
P	*E/P*	*P*	*E/P*	*P*	*E/P*
19.02	5.595	15.59	2.363	10.20	7.242
28.52	5.983	26.20	2.417	25.49	7.268
36.25	5.978	36.25	2.455	26.78	7.192
44.89	6.041			36.20	7.252
				45.12	7.243

Sulfate de Baryum I (baryte naturelle).

Un échantillon de baryte naturelle [1]) fut finement pulvérisé et les cristaux fins furent traités longuement par des grandes quantités d'eau bidistillée bouillante (500 cc de cette eau provenant du traitement de 10 gr de baryte ne donnaient après évaporation aucun résidu appréciable). Les cristaux secs furent soigneusement tamisés à deux reprises en isolant chaque fois la fraction traversant le tamis 20 et retenue sur le tamis 30 (30 mailles par cm). Ces cristaux se présentaient sous le microscope comme des blocs prismatiques réguliers d'un diamètre d'environ 150 μ. Nous avons mesuré le potentiel électrocinétique à leur surface en présence de solutions de chlorure de baryum et de sulfate de sodium. Les valeurs des potentiels n'étaient notés que lorsqu'ils restaient constants (trois mesures successives dans les deux sens). Les valeurs du tableau II montrent que les cristaux

TABLEAU II.

Baryte naturelle $P = 36.25$ cms Hg $\quad T = 18—21°$

Conc. (en μ moles/L.)		*E/P*	$K \times 10^6$ *)	ζ (en mvolts)
0	(H_2O)	—110.1	2.192	— 22.63
0	(H_2O)	— 98.0	2.510	— 22.97
0.1	$BaCl_2$	— 78.88	3.440	— 22.5
5	„	— 89.16	4.010	— 33.40
10	„	— 46.60	6.438	— 28.12
20	„	— 33.90	7.330	— 23.27
50	„	— 10.32	14.90	— 14.58
100	„	— 5.23	25.23	— 12.36
200	„	— 3.293	38.51	— 11.90
500	„	— 1.350	97.20	— 12.20
50	Na_2SO_4	— 16.92	15.43	— 25.0
100	„	— 12.93	29.85	— 35.45

*) K = Conductibilité spécifique à 25°.00

[1]) Que Mr. le Prof. SCHMUTZER nous a obligeamment cédé des collections de l'institut de minéralogie de l'Université.

de baryte restaient chargés négativement en présence d'une solution de chlorure de baryum et la courbe ζ/C (Fig. 1) est en tous points semblable à celle de la paraffine. Notons cependant que toutes les valeurs

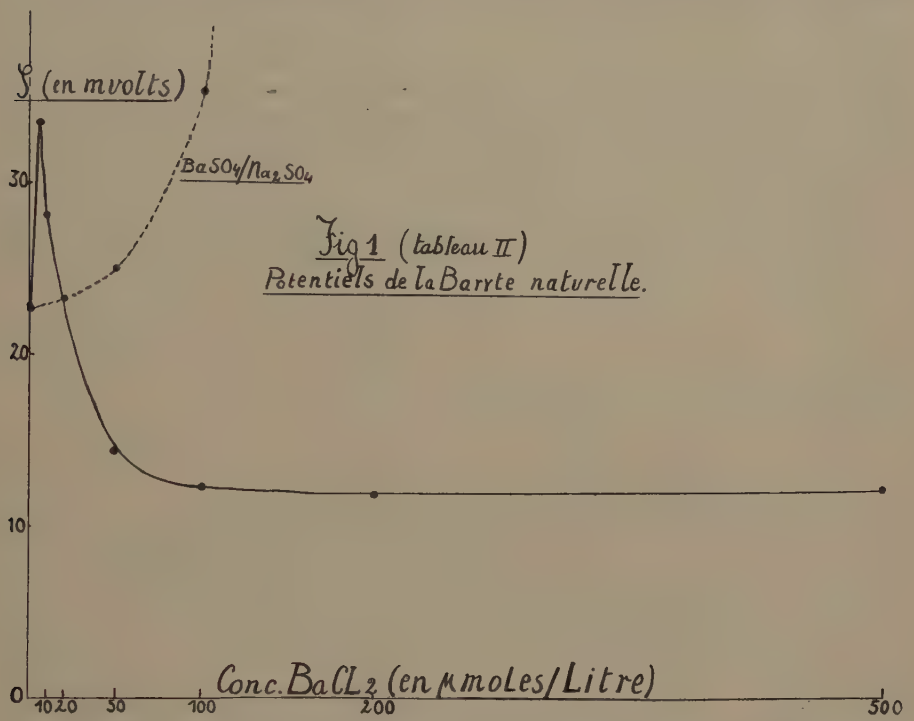

Fig 1 (tableau II)
Potentiels de la Barrte naturelle.

des potentiels pour la baryte naturelle sont abaissées dans un même rapport (environ 2½ fois plus faibles que celles de la paraffine). Cet abaissement est probablement dû à la conductibilité superficielle des cristaux, qui est dans ce cas-ci notablement plus grande que dans le cas d'un capillaire en paraffine unique, et n'est d'ailleurs pas (ou très peu) influencée par l'augmentation de la concentration en chlorure de baryum. (cfr. BRIGGS [1]).

Sulfate de Baryum II (précipité).

Les principaux résultats furent obtenus avec le sulfate de baryum II, obtenu par précipitation dans les conditions suivantes; une solution contenant 12.5 millimoles de $BaCl_2$/1000 cc et une solution contenant 15 millimoles de Na_2SO_4/750 cc étaient chauffées à l'ébullition et la solution de $BaCl_2$ était versée goutte à goutte et en agitant énergiquement, dans la solution de Na_2SO_4. La solution était digérée pendant trois heures à la température de l'ébullition, puis laissée en contact pendant 24 heures. Les cristaux étaient lavés jusqu'à absence complète de chlorures, puis agités pendant une semaine avec de l'eau de conductivité, qui était renouvelée toutes les douze heures [2]). Ce traitement prolongé évitait un vieillissement

1) D. BRIGGS, Coll. Sympos. Monogr. 6, 41, 1929.

2) La résistance spécifique du filtrat était constante dès le quatrième jour.

des cristaux pendant la durée des mesures consignées dans les tableaux III, IV, V et VI. Le sulfate de baryum ainsi lavé était conservé dans un vase bouché, en suspension dans quelques cc d'eau de conductivité. Ceci permettait de mesurer, à l'aide d'une micropipette, une même quantité de suspension pour la confection de chaque diaphragme. Vus sous le microscope, les cristaux étaient des aiguilles croisées à bords très découpés. Avant chaque mesure les cristaux de sulfate de baryum étaient laissés en contact pendant douze heures avec la solution de mesure et celle-ci était aspirée finalement pendant deux heures à travers le diaphragme.

En présence d'eau pure la charge des cristaux était négative : elle était positive dès les très faibles concentrations en chlorure de baryum (Fig. 2).

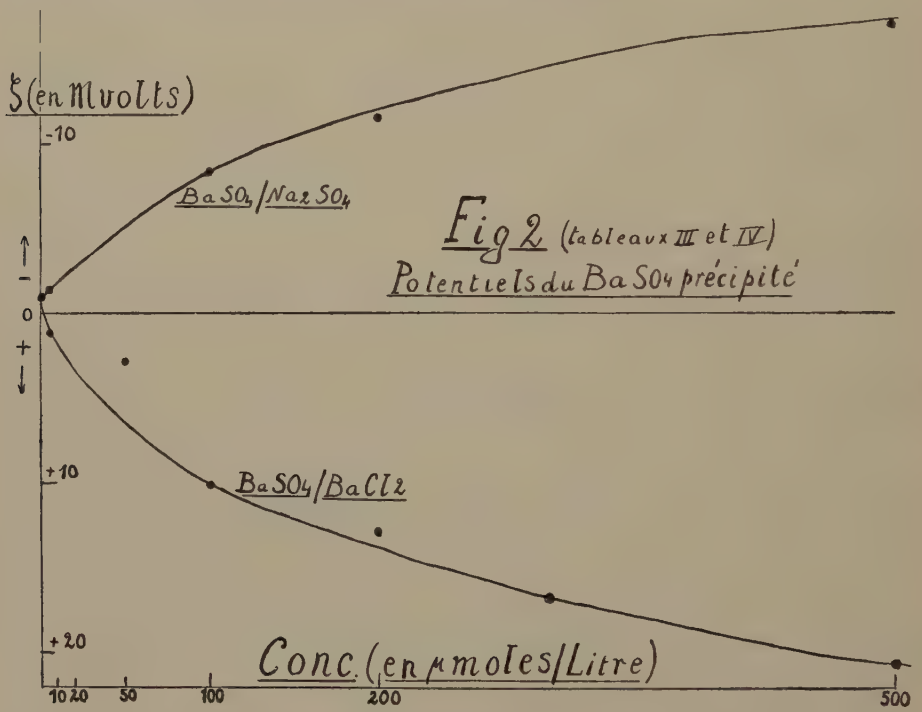

Fig 2 (tableaux III et IV)
Potentiels du BaSO4 précipité

TABLEAU III.

Sulfate de Ba II $P = 36.25$ cms Hg $T = 18-21°$

Conc. (en μ moles/L)	E/P	$K \times 10^6$	ζ (en mvolts)
0 (H_2O)	— 4.550	2.192	— 0.93
5 $BaCl_2$	+ 2.922	4.398	+ 1.20
10 „	+ 4.647	6.44	+ 2.80
50 „	+ 2.582	14.90	+ 3.61
100 „	+ 3.768	28.92	+ 10.2
200 „	+ 3.56	38.52	+ 12.83
300 „	+ 3.819	47.14	+ 16.86
500 „	+ 2.209	97.20	+ 20.61

et la courbe montre un accroissement asymptotique des potentiels avec la concentration.

En présence de sulfate de sodium la charge était négative, et la courbe était presque symétrique à celle du chlorure de baryum. Une concentration de 100 μ moles de $ZnSO_4/L$ donnait également une charge négative aux cristaux ($\zeta = -5.22$ m volts). Nous devons admettre ici également une adsorption préférentielle de l'ion SO_4'', et qui l'emporte sur l'action positivante de l'ion Zn^{++} (voir plus loin).

TABLEAU IV.

$BaSO_4$ précipité (II) $P = 36.25$ cms Hg $T = 18-21°$

Conc. (en μ moles/L)	E/P	$K \times 10^6$	ζ (en mvolts)
5 Na_2SO_4	— 2.44	6.34	— 1.4
100 „	— 2.725	32.80	— 8.4
200 „	— 2.679	45.57	— 11.5
500 „	— 1.515	117.0	— 16.6

Dans une seconde série nous avons mis en oeuvre les chlorures de métaux bivalents (tableau V) qui tous chargent positivement:

TABLEAU V.

$BaSO_4$ précipité (II) $P = 36.25$ cms Hg $T = 18-21°$

Conc. (en μ moles/L)	E/P	$K \times 10^6$	ζ (en mvolts)
2 $SrCl_2$	2.622	5.210	+ 1.2
10 „	2.891	7.48	+ 2.1
100 „	1.970	27.38	+ 5.0
2 $CaCl_2$	2.281	5.84	+ 1.2
10 „	4.238	6.274	+ 2.5
100 „	1.675	25.97	+ 4.1
3 $MgCl_2$	— 1.800	4.268	— 0.72
10 „	2.408	5.931	+ 1.29
100 „	0.845	29.62	+ 2.3
10 $ZnCl_2$	2.69	8.24	+ 2.0
100 „	0.985	34.42	+ 3.1
100 $CdCl_2$	0.526	31.38	+ 1.5

TABLEAU VI.

100 KCl	— 2.921	19.52	— 5.4
100 NaCl	— 2.184	18.50	— 3.8

Si les potentiels électrocinétiques sont une mesure du taux d'adsorption, les différents métaux se placeraient dans l'ordre suivant : Sr > Ca > Zn > Mg > Cd, en prenant comme point de comparaison les concentrations de 100 μ moles). Suivant les expériences de TEZAK [1]) le zinc serait en réalité moins adsorbé que le Mg et le Cd. Nous devons admettre ici l'existence d'un facteur étranger qui augmenterait le potentiel positif en présence de chlorure de zinc.

Les chlorures des métaux alcalins (tableau VI) accusent au contraire une charge négative très nette, qui est plus prononcée pour le potassium que pour le sodium.

En présence de solution de chlorure d'aluminium, le sulfate de baryum présentait une charge positive très nette. (Tableau VII ; Fig. 3).

TABLEAU VII.

$BaSO_4$ précipité (II) $P = 36.25$ cms Hg $T = 18—21°$

Conc. (en μ moles)	E/P	$K \times 10^6$	ζ (en mvolts)
0 (H_2O)	— 3.378	4.980	— 1.6
10 $AlCl_3$	1.741	8.051	+ 1.3
50 „	1.56	26.38	+ 3.8
100 „	0.990	48.83	+ 4.5
200 „	1.050	80.91	+ 7.9
500 „	0.446	198.9	+ 8.3

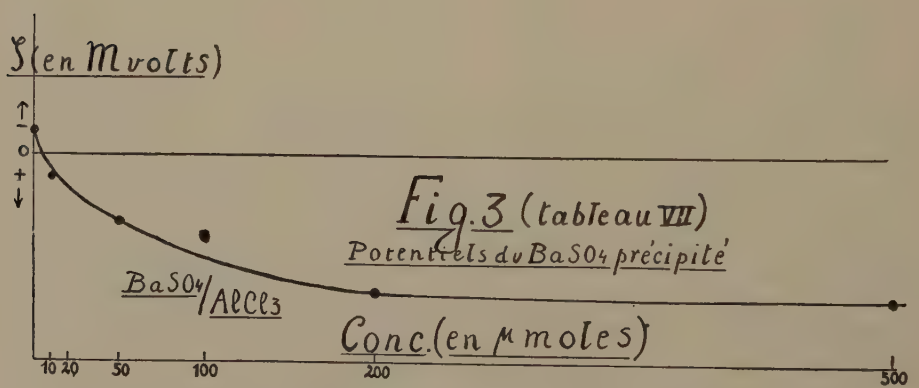

Fig. 3 (tableau VII)
Potentiels du $BaSO_4$ précipité

En reprenant les valeurs des potentiels avec un même $BaSO_4$, qui était resté en contact avec de l'eau pure dans le flacon de réserve, les cristaux accusaient un potentiel positif très net : $E/P = + 3.6$, 4.0, et la solution renfermait des traces minimes de chlorures. Les cristaux de sulfate de

[1]) Loco Cito.

baryum furent agités de nouveau pendant quatre jours avec de l'eau de conductivité (avec six renouvellements) et après ce temps leur charge était redevenue négative $E/P = -3.3, -3.1$.

Nous n'avons pu expliquer ce renversement de signe que par un passage en solution du chlorure de baryum, occlus dans les cristaux ou fixé par adsorption interne suivant BALAREW [1]).

Voulant obtenir certitude concernant la charge d'équilibre avec l'eau pure nous avons fait varier les conditions de préparation. Le *$BaSO_4$ III* fut préparé des deux solutions: 1° 50 mmoles de K_2SO_4 au litre et 2° 50 mmoles de $BaCl_2$ au litre. Le sulfate était versé dans le chlorure de baryum à l'ébullition. Le précipité fut digéré, filtré, lavé et agité de même pendant dix jours. Les cristaux étaient très fins et accusaient dans l'eau pure une charge négative très nette: $E/P = -3{,}29$; $-4{,}87$.

Le $BaSO_4$ IV fut préparé de même de 15 mmoles $BaCl_2$/litre et de 12.5 mmoles K_2SO_4/litre. Le sulfate fut versé dans le chlorure de baryum. La charge négative des cristaux était très nette, $E/P = -2{,}45$.

Enfin, pour éviter autant que possible toute occlusion des ions, un échantillon de $BaSO_4$ fut préparé de la façon suivante: 10 litres d'eau distillée furent chauffés à l'ébullition et soumis à une rapide agitation mécanique. De deux burettes parallèles on laissait couler goutte à goutte et avec une même vitesse les solutions de sulfate de potassium et de chlorure de baryum, exactement dixième normales. Les cristaux étaient traités comme précédemment. Ils se présentaient sous forme de prismes rhombiques à côtés lisses et réguliers d'un diamètre variable de 20 à 40 μ. Les valeurs du tableau VIII montrent que le développement de la surface active de ce $BaSO_4$ pouvait être considéré comme intermédiaire de celui des formes I et II. Les cristaux avaient dans l'eau pure un potentiel de signe négatif de —6,6 mvolts et conservaient leur charge négative en présence de 100 μ moles de $BaCl_2$ (tout comme le $BaSO_4$ I). Voulant vérifier s'il ne s'agissait pas ici d'une forme allotrope, le $BaSO_4$ IV et le $BaSO_4$ II furent soumis à l'ana-

TABLEAU VIII.

$BaSO_4$ précipité $P = 36.25$ cms Hg $T = 18$—$20°$

Conc. (en μ moles/L)	E/P	$K \times 10^6$	ζ (en mvolts)
0 (H_2O)	— 47.50	1.483	— 6.602
10 $BaCl_2$	— 15.14	6.441	— 9.11
10 „	— 15.51	6.445	— 9.37
100 „	0.596	28.92	— 1.61
100 „	0.633	28.92	— 1.73
100 „	0.591	28.90	— 1.60

[1]) D. BALAREW, Koll. Beih. **30**, 249, 1930.

lyse spectrale aux rayons X. Les deux spectrogrammes étaient absolument identiques et les Θ, calculés sans correction d'épaisseur de préparation, correspondaient parfaitement aux valeurs de WAGNER [1]), pour la baryte naturelle.

Nos différents échantillons de $BaSO_4$ avaient donc bien la même structure cristalline et seul le degré de développement de leur surface active était différent.

Afin d'approfondir encore d'avantage la question, nous avons tenté de construire des capillaires uniques par compression. La méthode consistait à comprimer la poudre dans un cylindre, autour d'un fil d'acier serré. Ces tentatives échouèrent complètement avec les $BaSO_4$ décrits plus haut, et ne réussirent qu'avec des cristaux à grains très fins, tels que ceux d'un sulfate de baryum pour „Röntgenographie" Merck.

Ces capillaires avaient une longueur de 7 cms environ et leur diamètre interne variait de 0,2 à 0,3 mm. Leur vitesse d'écoulement, sous la pression d'expérience y était d'environ de 2,25 cc par minute. Ces capillaires étaient sans charge sur toute l'échelle des concentrations en chlorure de baryum. Nous avons réussi à monter un diaphragme très serré de ce même sulfate de baryum: le potentiel d'écoulement était nul en présence de 100 μ moles de $BaCl_2$ litre.

Résumé.

1. Les potentiels d'écoulement du sulfate de baryum sont fonction du développement de la surface des cristaux.

2. Les cristaux de baryte naturelle avec un faible développement de surface conservent une charge négative même en présence du chlorure de baryum et la courbe ζ/C est analogue à celle de la paraffine. Les valeurs des ζ sont abaissées uniformément par la conduction de surface.

3. Le sulfate de baryum précipité, dont les dimensions des grains sont d'environ 10 μ, présente dans l'eau pure une faible mais nette charge négative. Il se charge positivement par le $BaCl_2$ et négativement par les sulfates solubles, les courbes ζ/C sont presque symétriques dans les deux sens.

4. Les chlorures des métaux bivalents Sr, Ca, Mg, Zn, Cd, lui communiquent un potentiel de signe positif, tandis que ceux des métaux alcalins (Na et K) augmentent le potentiel de signe négatif de la paroi.

Octobre 1934. *Laboratoire van 't Hoff, Université d'Utrecht.*

Ces travaux ont été faits grâce à un subside accordé à l'un de nous (R. RUYSSEN) par le Fonds National de la Recherche Scientifique de Belgique, auquel nous adressons nos vifs remercîments.

[1]) WAGNER, Z. Physik. Chem. (B) 2, 27, 1929.

Plantkunde. — *De Grenzen der Bloeibaarheid bij Bol-Irissen.* (Eerste stuk). Door A. H. BLAAUW. (Meded. N°. 41 van het Laboratorium voor Plantenphysiologisch Onderzoek te Wageningen).

(Communicated at the meeting of October 27, 1934).

Wanneer een bol-iris, bijv. de variëteit *Imperator*, omstreeks 1 Augustus uit den grond wordt genomen, oogsten wij van elke iris die *niet* gebloeid heeft één vrij grooten, centralen, d.w.z. terminalen ronden bol, en daarnaast bijv. 1 tot 4 kleinere aan een zijde min of meer afgeplatte bolletjes[1]). Ik heb thans in dit stuk verder alleen het oog op de centrale ronde bollen, waarvan wij den oogst verder sorteeren naar den omtrek in bollen van 4—5 cm, van 5—6 cm, van 6—7 cm, enz.

Een bol van 6—7 cm omtrek bestaat in den regel uit 2, soms 3 vliezige hulsels, de bases der afgestorven loofbladen; verder uit meestal 3 dikke rokken, dicht met zetmeel gevuld; daarbinnen treft men 3, soms 2, scheedebladen aan in jongen toestand. Daarna komen de nieuw-aangelegde loofblaadjes; in bollen van 5—6 en van 6—7 cm omtrek zijn er ± 1 Augustus in den regel 3 loofblaadjes aangelegd met dikwijls het begin van het 4e. Daarbinnen ligt het terminale bladvormende vegetatiepunt.

In 1932 werden op 21 Juli bollen van Imperator van 6—7, van 5—6 en van 4—5 cm in verschillende temperaturen gelegd tot 19 October. Toen werden van deze bollen eenige in alcohol gefixeerd en weer onderzocht, terwijl de overige werden geplant.

Wat er tijdens de zomerbehandeling gedurende 3 maanden dan uiterlijk is vast te stellen, vindt men in Tab. 1 en 2, waarbij de toestand van nieuwe knopjes hier voorloopig buiten beschouwing blijft.

Spreken wij hier van een gemiddeld aantal blaadjes van bijv. 3.05, dan is dit 1×2, 6×3 en 3 maal $3\frac{1}{2}$, d.w.z. bij 3 ex. is het vierde in aanleg. De 2e decimaal heeft hierbij nauwelijks beteekenis en zou weggelaten kunnen worden. — Zoo is bijv. 5.50 (17°): 3×5, — $2 \times 5\frac{1}{2}$ en 3×6 bladen. De samenstelling van dergelijke gesorteerde Iris-bollen is uiterst gelijkmatig.

Als het belangrijkste valt bij Tab. 1 en 2 het volgende op te merken:

1°. Het proces der bladvorming gaat in een uiterst traag tempo.

2°. De temperaturen, die wij tot nu toe bij andere gewassen hebben leeren kennen als de snellere voor orgaanvorming, welke toch met cel-

[1]) Voor de beschrijvingen, raadplege men nader de binnenkort verschijnende publicatie „De periodieke ontwikkeling van de bol-iris."

TABEL 1.

Imperator, 6—7 cm. Het gemiddelde aantal en de lengte der aangelegde loofblaadjes (L 1, L 2, enz.) in mm vóór en nà verschillende zomerbehandeling.

		Aantal	L 1	L 2	L 3	L 4	L 5
21 Juli '32		**3.05**	0.99	0.54	0.29		
19 Oct.	20°	**5.12**	5.12	2.91	1.28	0.49	
	17°	**5.50**	8.04	4.31	1.92	0.62	0.40
	13°	**6.19**	19.06	6.7	2.5	0.86	0.54
	11°	**6.19**	19.12	6.19	2.25	0.84	0.51
	9°	**5.56**	13.81	4.56	1.36	0.65	0.39

TABEL 2.

Imperator, 5—6 cm. Het gemiddelde aantal en de lengte der aangelegde loofblaadjes (L 1, L 2, enz.) in mm vóór en nà verschillende zomerbehandeling.

		Aantal	L 1	L 2	L 3	L 4	L 5
21 Juli '32		**3.25**	1.00	0.52	0.32		
19 Oct.	23°	**3.94**	3.08	1.34	0.52		
	20°	**4.86**	5.76	2.86	1.12	0.41	
	17°	**4.93**	7.63	3.90	1.44	0.54	
	13°	**5.75**	17.50	6.09	0.65	1.89	0.39

deeling gepaard moet gaan, werken juist zeer traag (17° en hooger steeds trager). *Bij 11° en 13° ligt het optimum van de bladvorming.* Wij zullen in latere publicaties dit met veel meer gegevens staven. Thans moeten wij voor dit artikel eerst dit feit op zichzelf kennen. Intusschen bereikt deze Iris haar optimale praestatie voor de bladvormende periode door den aanleg van ± 3 jonge blaadjes in 3 maanden. In 23° C. wordt nauwelijks één nieuw blad er bij gevormd. Ook bij 9° C. is de bladaanleg reeds iets trager. Dat is uiterlijk alles, wat den aanleg betreft, naast een eveneens uiterst langzame knopvorming in den oksel van bepaalde bladdeelen. Maar deze langzame orgaanvorming en dit lage optimum van de bladvorming is reeds een zeer opmerkelijk verschijnsel voor de vergelijkende experimenteele morphologie.

3°. Wat de lengtetoeneming dezer blaadjes betreft, ook hiervoor ligt het directe optimum in deze maanden bij 11° tot 13° C. Dit valt het meest in het oog bij het belangrijke 1e loofblad, maar treedt toch bij alle andere jonge blaadjes evenzeer op den voorgrond.

Nu zei ik hierboven, dat *uiterlijk* omtrent den orgaanaanleg tijdens die zomerbehandeling niet anders viel te constateeren (afgezien dus van de zijknopjes) dan de aanleg van ± 3 jonge blaadjes in 3 maanden bij de vlugste temperaturen. Maar inwendig moet er iets veel gewichtigers hebben plaats gegrepen. Worden de aldus behandelde bollen buiten geplant — dus onder thans verder gelijke omstandigheden —, dan is het volgend jaar het effect der temperaturen bij voorbeeld gelijk Tab. 3 in 't kort toelicht.

TABEL 3.

Bloei Imperator 1933 bij verschillende grootte na verschillende behandeling in den zomer van 1932.

	Behandeling 21 Juli—19 Oct 1932	Zomer 1933			
		Procent *Niet-Bloeiers*	Aantal loofbladen	Procent *Bloeiers*	Aantal loofbladen
6—7 cm.	20°	0	—	100	7.90
(4.86 gr.)	17°	0	—	100	8.17
$n = 50$	13°	5	—	95	8.87
	11°	39	2.7	61	8.09
	9°	$67^1/_2$	2.4	$32^1/_2$	7.62
5—6 cm.	23°	10	3.2	90	7.05
(3.42 gr.)	20°	20	3.45	80	7.65
$n = 100$	17°	19	3.5	81	7.97
	13°	23	3.2	77	8.28
4- 5 cm.	23°	66	3.0	33	7.00
(2.29 gr.)	17°	91	2.9	9	—
$n = 32$					

Wij weten, dat de primordia van de bloem zelve bij Imperator op het veld pas in Maart, op z'n vroegst einde Februari worden aangelegd (Zie Mededeel. N°. 38). *Toch is in October na de zomerbehandeling, reeds beslist, of de bol over ± 4 maanden een bloem zal gaan aanleggen, of opnieuw een jongen ronden bol zal vormen.*

De gegevens van Tab. 3, die door vele andere voorbeelden nog verder bevestigd zullen worden, toonen direct aan, dat de temperatuurbehandeling in Aug.-Sept.-Oct. bij zekere grootten van bol het bloei-procent in hooge mate beïnvloedt. Bij die grootten 6—7 en vooral 5—6 cm voor Imperator heeft men het in de hand te bewerken, dat 100 % of nagenoeg 100 % zal

bloeien òf dat dezelfde grootte slechts voor een gering procent zal bloeien. Bijv. bij 6—7 cm bloeit na 3 maanden 17° en 20° de volle 100 % en slechts 32 % na 9° ; bij 5—6 cm werd in 1932 niet lager dan 13° behandeld ; in 1933 behandeld met 9° en 5° leverde 5—6 cm in 1934 resp. 12 % en 0 % bloeiers, na 23° 95 % bloeiers (zie Tab. 4).

Terwijl wij later met uitvoeriger proeven op dit verschijnsel terugkomen, hebben wij hiermee enkel een voorbeeld gegeven van het vraagstuk dat ons botanisch interesseert. Terwijl daarmee een der belangrijkste vragen samenhangt voor een rationeele cultuur der Bol-Irissen.

Of Iris-bollen zullen bloeien of niet, hangt *allereerst af van hun omvang* (gewicht), in de *tweede* plaats van de zomerbehandeling. De vereischte omvang voor bloeibaarheid is dan bij de verschillende soorten en variëteiten nog weer zeer uiteenloopend. Blijven wij eerst bij „Imperator" dan kan men zeggen, dat 8—9 cm, maar ook 7—8 cm na behoorlijke zomerbehandeling voor 100 % buiten bloeit, — dat beneden 4 cm slechts zelden een bol het volgend jaar bloeit. De grootste bollen worden dus in hun bloeivermogen zeer weinig door de temperatuur beïnvloed ; de kleine bollen van 4 cm en lager zijn door geen hoogere temperatuur tot bloei te brengen. Maar al de bollen van ± 4 cm tot ± 7 cm omtrek kunnen *bloeiers of niet-bloeiers* worden. Dit wordt in sterke mate door de temperatuurbehandeling in de zomermaanden beïnvloed.

Dergelijke bollen staan dus *op de grens,* zij zijn lastig voor de cultuur, maar interessant om het probleem. Bij het rooien in den zomer (zie Tab. 1 en 2) hebben zij 2 tot 4 loofblaadjes in aanleg gereed en nu kunnen zij twee verschillende wegen inslaan.

A. Zij bloeien het volgend jaar *niet* : het aantal in den zomer reeds aangelegde loofblaadjes, is het aantal in het volgende voorjaar assimileerende, grondstandig blijvende loofbladen. De later in den zomer, het winterhalfjaar en het voorjaar aangelegde blaadjes vormen een nieuwen centralen ronden bol ; de eerste 3 tot 4 daarvan worden tot dikke rokken. Bij het opnieuw vormen van dezen terminalen bol blijft de jonge knop in den oksel van het eerste loofblad vrij onaanzienlijk.

B. Zij bloeien het volgend jaar *wel* : van de bij het rooien reeds aangelegde loofblaadjes, blijft *alleen het onderste in den regel grondstandig.* De later aangelegde blaadjes vormen nu geen bol, maar worden evenals de reeds aanwezige tot stengelbladen, assimileeren dus allen in het komende jaar. Het aantal loofbladen bedraagt bij bollen van 6—7 cm meestal 7 tot 9 (Tab. 3 laatste kolom). De 2 bovenste bladen (zonder internodium) behooren functioneel evenzeer tot de loofbladen als zij morphologisch als spathabladen tot de bloeiwijze gerekend kunnen worden. Het eindgroeipunt wordt dus thans verbruikt door bloemstengel en loofbladen : nù ontwikkelt zich de jonge knop in den oksel van het ééne grondstandige loofblad tot den grootste der nieuwe bollen. Deze hoofdbol, vergelijkbaar met den grootsten jongen bol van een tulp, die gebloeid heeft,

is vrij wat anders van vorm dan een centrale (eindelingsche) ronde bol, doordat hij aan de zijde van den bloemstengel door den druk platter is en eenigszins een groeve vertoont.

Af en toe doet zich een variant voor van geval B; het kan gebeuren dat de aangelegde bloemstengel reeds vroegtijdig weer te gronde gaat, dan zal de groote nieuwe zijbol beter centraal zitten, ronder zijn, daar de stengel niet gehinderd heeft. In die gevallen is het vaak moeilijk te beslissen of het een „platte" zijbol of een „ronde van niet-gebloeid" is.

Na niet-bloeien *oogst* men een ronden dikkeren bol naast enkele kleine platte bolletjes. Na bloeien oogst men een afgeplatten grooten hoofdbol met enkele van binnen naar buiten in grootte afnemende kleinere bolletjes.

Nu worden voor de levering van bloeibare bollen alleen *ronde* bollen (van voldoenden omvang) geëischt. De vraag rijst: „Is er eenig werkelijk bezwaar, dat behalve ronde bollen evenzeer die groote afgeplatte bollen van gelijk gewicht worden afgeleverd? Zelfs is de vraag gewettigd, waarom bol-irissen, zooals de Spaansche en de Hollandsche, niet in den regel, evenals tulpen en evenals trouwens bij de Engelsche irissen geschiedt, bij den kweeker mogen bloeien, zoodat als regel de „platte" bollen van voldoenden omvang zouden geleverd worden.

Dit geeft allereerst aanleiding tot de proef: Is de bloeibaarheid van even zware ronde en platte bollen gelijk te noemen? Wij hebben hierover enkele proeven genomen, die pas later na herhaling zullen besproken worden (zie Tab. 5). Daaruit blijkt totdusver, dat bij een gelijk gewicht zulke platte bollen in bloeivermogen niets of zeer weinig ten achter staan bij de gewenschte ronde (zie dezelfde meening bij GRIFFITHS 1928).

Wij hebben echter wel den indruk gekregen, dat de zware platte bollen zeer voldoende bloeien, maar niet zoo licht de volle 100 % bereiken als even zware ronde bollen.

Maar de platte hoofdbollen worden — zonder buitengewone voorzorgen — voor een belangrijk deel ziek en lijden veel sterker bij de verzending. Ook in onze partijtjes kwam een aanzienlijk percentage in het geheel niet op. In de periodieke ontwikkeling van de bol-iris zullen wij nader de oorzaken hiervan toelichten (zie ook GRIFFITHS 1928). Wij moeten hier volstaan met de erkenning dat er alle reden is bij de leverantie voor *ronde* bollen te zorgen. Die ronde bollen moeten daarbij voor nagenoeg 100 % het volgend jaar bij den afnemer bloeien. De moeilijkheid is nu, dat zekere grootten, bijv. bij *Imperator* liggende tusschen ± 4 en ± 7 cm omtrek, ten deele bloeien, ten deele niet bloeien. Men moet dus streven naar een zoodanige behandeling, dat de kleinere bollen, die de kweeker weer plant om er grootere ronde bollen van te oogsten, voor nagenoeg 100 % *niet* bloeien; — en dat de grootere, die hij verkoopt, in den zomer zóó behandeld worden, dat zij voor nagenoeg 100 % wèl zullen bloeien.

Voor de toepassing van dit vraagstuk gaat het er dus om, òf er tweeërlei behandeling voor bepaalde bolgrootten van verschillende variëteiten te

vinden en aan te geven is, waardoor aan beide doeleinden voldaan wordt, zooal niet voor 100 % dan althans zoo goed mogelijk.

Naast de practische benadering van het vraagstuk, waarmee wij ons hier in hoofdzaak bezighouden, blijft de botanische zijde van dit onderwerp ons voornamelijk voor de vraag stellen, hoe het mogelijk is, dat bij bollen, die op de grens staan van niet- of wel- kunnen bloeien, de behandeling in den zomer reeds den doorslag geeft, of de verder te vormen blaadjes eveneens loofbladen òf reserveorganen zullen worden, of tusschen de blaadjes al of niet internodiën zullen gevormd worden, die tot een stengel kunnen uitgroeien, en of dus ten slotte 4 à 5 maanden nà de zomerbehandeling al of niet bloemen worden aangelegd.

Wij willen thans nog aan dit eerste stuk in de tabellen 4—8 eenige uitkomsten toevoegen omtrent bloeien en niet bloeien in 1934 in aansluiting aan Tab. 3 van 1933.

Naast de hybride *Imperator,* werden ook *Rembrandt* en de voor vroegen trek belangrijke *Wedgewood* in het onderzoek opgenomen en naast deze Hollandsche irissen ook de in Marocco veel voorkomende *Iris tingitana,* die door kruisingen deel heeft aan het ontstaan van verschillende hybriden van de groep der Hollandsche irissen. Het aantal exemplaren (*n*) per proef in 1932 en 1933 is betrekkelijk klein geweest (meestal 40 tot 100 stuks), maar deze bollen worden dan ook naar omtrek en gewicht stuk voor stuk uitgezocht. Het is onvoldoende alleen met omtrekken rekening te houden, daar vooral bij kleinere z.g. ronde bollen de omtrek op de breedste plaats dikwijls niet zuiver rond is èn de bol in zeer verschillende mate door de kleine zijbollen aan de basis kan afgeplat zijn. Voor een goede vergelijking is het gewicht alleen een goede basis, terwijl de omtrekken, zooals dit bij de groote massa in de praktijk gebruikelijk is, fouten opleveren, die gering schijnen, maar juist op de grens van bloeien of niet-bloeien zeer nauw steken en die daardoor de sorteering minder zuiver maken.

Er was verder in 1933—'34 veel ziekte in verschillende groepen, waardoor ons kleine aantal vaak ernstig benadeeld werd. De zieke bollen, waaraan bloeier of niet-bloeier niet was te onderscheiden en die dikwijls, vooral bij de platte bollen van *Imperator,* in het geheel niet opkwamen, zijn in de tabellen *tusschen haakjes* geplaatst en zijn buiten de berekening van het bloeiprocent gelaten. Zoo vertoont 23° C. voor die temperatuur abnormaal veel ziekte daar in één thermostaat een ernstige infectie met luizen zich ontwikkeld had. Belangrijk is het wellicht er op te wijzen dat 17° C. en 20° C. de minste zieken en het mooiste gewas hadden, ook beter en minder ziekte dan bijv. 25½° C., als wij nu 23° C. even terzijde laten.

Van *Iris tingitana,* uit Z.-Frankrijk ontvangen stonden ons slechts weinig bollen ter beschikking voor deze proeven. Ondanks deze bezwaren vermelden wij toch reeds deze resultaten omdat zij in de hoofdzaken volkomen aansluiten aan Tab. 3 van 1932—'33.

Als men het volgend jaar zoo veel mogelijk bloei wil hebben, m.a.w. voor de bollen die de kweeker wil afleveren, is bij voldoende flinke maten een behandeling met 17° C. tot 20° C. aan te bevelen. Zie Tab. 3 en 4: *Imperator* reeds bij 6—7 cm voor 98 en 100 % bloeiend na 17° (zelfs na 13° C.); Tab. 6 *Wedgewood* 8—9 cm 100 % na 17° C.; — 97 % na 20° C.

Dit is echter de uitkomst op het terrein buiten. Maten, die bij 17°—20° C. voor 100 % bloeien op 't veld, zullen bij langzame en vooral bij sterken trek waarschijnlijk meer niet-bloeiers vertoonen. Of zij echter slechter zouden bloeien dan na behandeling met 23° C. zullen wij bij de proeven over snellen bloei van bol-irissen nader uitmaken. Voorloopig is er geen reden dit aan te nemen.

Naarmate echter de zwaarte der bollen geringer is, des te noodiger is het minstens 23° C. gedurende langen tijd te geven, wanneer men wèl-bloeien wil verzekeren. M.a.w. als men *dichter bij de grens* van niet-bloeien komt, wat de bolzwaarte betreft, dàn geven 17° C. en 20° C. een geringer bloei-percentage dan 23° C. Vergelijk Tab. 3 tot 8.

Hoogere temperaturen zooals 25½° C. en 28° C. bieden voor zoover ik thans kan beoordeelen geen voordeel boven 23° C. In 28° C. daalt het bloeiprocent nagenoeg altijd, d.w.z. bij behandeling gedurende 2 of 2½ maand. Of bij trekken een korte behandeling van 3 tot 5 weken in 28° C. of in 23° C. of lager beter is, behandelen wij elders. Ook 25½° C. geeft meestal geen beter bloeiprocent dan 23° C., terwijl dan in andere opzichten voor 't verdere gewas de lagere temperatuur de voorkeur verdient.

Dus voor bloei beware men de flinke maten bij 17° C. tot hoogstens 20° C.; de twijfelmaten bij 23° C. Daarbij is ± *23° C. de optimale vóór-temperatuur voor den achteraf plaatsvindenden bloemaanleg.*

Bloeiprocent in 1934 na zomerbehandeling 1933.

TABEL 4.

Imperator, ronde bollen, behandeld 18 Aug.—23 Oct.

	4—5 cm. 2.03 gr. $n = 50$	5—6 cm. 3.54 gr. $n = 47$	6—7 cm. 5.42 gr. $n = 50$
28°	9 % (15)	81 % (4)	—
25½°	36 % (12)	79 % (2)	—
23°	43 % (8)	95 % (5)	—
20°	—	—	—
17°	21 % (5)	—	98 % (0)
13°	—	47 % (4)	100 % (3)
9°	—	12 % (6)	83 % (2)
5°	—	0 % (8)	76 % (6)

TABEL 5.

Imperator, zij-bollen, behandeld 4 Aug.—20 Oct.

	4.1 cm. 1.5 gr. $n = 40$	5.5 cm. 3.3 gr. $n = 40$	6.3 cm. 4.7 gr. $n = 40$
23°	0 % (8)	78 % (11)	90 % (19)
17°	0 % (3)	71 % (5)	93 % (11)

TABEL 6.

Wedgewood, behandeld
11 Aug.—20 Oct. '33.

	6—7 cm. 6.88 gr. $n = 50$	8—9 cm. 13.5 gr. $n = 50$
28°	58 % (7)	92 % (1)
25½°	94 % (4)	89 % (3)
23°	89 % (5)	85 % (7)
20°	72 % (0)	97 % (3)
17°	62 % (2)	100 % (2)
13°	14 % (1)	40 % (2)
9°	4 % (2)	13 % (2)

TABEL 7.

Rembrandt, behandeld 22 Aug.—24 Oct. '33.

	6.1 cm. 4—5 gr. $n = 24$	5.5 cm. 3—4 gr. $n = 48$	4.8 cm. 2—3 gr. $n = 38$
28°	ziek (16)	69 % (16)	23 % (11)
25½°	ziek (16)	54 % (22)	42 % (16)
23°	ziek (19)	78 % (10)	41 % (10)
20°	94 % (7)	61 % (1)	27 % (5)
17°	91 % (1)	67 % (6)	33 % (4)
13°	53 % (7)	14 % (9)	0 % (4)
9°	6 % (5)	3 % (12)	0 % (12)

TABEL 8.

Iris tingitana Juliana Rossi, behandeld 31 Juli—20 Oct.

	8—9 cm. 12.96 gr. $n = 25$	9—10 cm. 17.2 gr. $n = 25$
28°	32 % (6)	85 % (5)
25½°	80 % (10)	100 % (4)
23°	83 % (13)	100 % (14)
20°	14 % (4)	88 % (4)
17°	5 % (5)	35 % (2)

Moeilijker is nu de vraag der behandeling voor het niet-bloeien der bollen, die bij den kweeker nog een jaar moeten groeien tot zwaarder bollen. En dit is juist het belangrijkste punt voor deze iris-cultuur. De gegeven tabellen wijzen reeds den weg. Bollen, die na hoogere temperaturen voor 90 % bloeien (op het veld), zullen na behandeling met 9° C. of zelfs 5° C. slechts voor 10 % en minder bloeien. Tab. 4 *Imperator* 5—6 cm; tab. 6 *Wedgewood* 6—7 cm en 8—9 cm; tab. 7 *Rembrandt* 6.1 cm; (*Iris tingitana* 8—9 cm).

Hier schorten wij ons oordeel nog op in afwachting van uitvoeriger en ander soort behandelingen, die thans in dit jaar loopende zijn. Wij weten wel, dat wij beneden 13° C., dus bijv. met 9° C. den lateren bloemaanleg buitengewoon sterk reduceeren kunnen bij de twijfelmaten. Dit feit te weten is voor ons botanisch reeds van veel belang. Voor de toepassing komt daarbij dan echter de practische vraag, of men door die behandeling met bijv. 9° C. of lager het volgend jaar een voldoende dikte-toeneming krijgt; dus of deze bollen, die nù op de bloeibaarheidsgrens staan, het volgend jaar dan bollen opleveren, die wel voor 100 % bloeibaar zijn na 17° C. tot 20° C.

Deze gewichts- en dikte-toeneming en het aantal assimileerende bladen in de proeven van tab. 3 tot 8 zal ik vermelden in het tweede stuk waarin dit onderwerp wordt voortgezet.

Wageningen, 22 October 1934.

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RÉSUMÉ.

Des limites de l'aptitude à former fleurs chez les Iris bulbeux. (1re partie).

Les Iris bulbeux, tels que les hybrides hollandais et espagnols, les variétés de l'*Iris tingitana* du Maroc, nous proposent des problèmes bien intéressants, tant au point de vue scientifique que par leur importance pour la culture. L'hybride hollandais *Imperator*, par exemple, d'un contour de 8 cm. à l'arrachage, fleurira sans difficultés l'année suivante, et la température reçue par lui l'été précédent se trouvera n'avoir eu, dans des limites relativement reculées, qu'une influence minime. Inversement, les petits bulbes de moins de 4 cm. ne fleuriront presque jamais, quelque température que nous leur ayons donnée auparavant. Mais très intéressants, bien que difficiles pour la pratique, sont les bulbes qui se trouvent *sur la limite de l'aptitude à fleurir, comme l'Imperator* 5—6 cm., le *Wedgewood* 6—7 et 7—8 cm. Tandis que *l'Imperator*, en Hollande, ne constitue ses fleurs qu'en février-mars, le traitement de la période août-octobre *de l'année précédente* a déjà décidé si ces bulbes fleuriront ou non. Pendant ces mois d'été, il ne se forme dans le bulbe que de 1 à 3 jeunes feuilles (Tabl. 1 et 2). La température optimale de cette formation se situe de 11 à 13 degrés. Cette température vaut au bulbe une moyenne de trois nouvelles feuilles en trois mois, une température de 23 degrés ne lui en vaudra qu'une seule.

De même, l'accroissement en longueur est le plus rapide de 11 à 13°, fait qui ne peut nous étonner quand on sait le comportement des Tulipes et des Jacinthes, — mais qu'une température de 11 à 13° soit la température optimale pour la formation d'organes (donc : pour la division de cellules) est un fait que jusqu'à présent, nous n'avions pas constaté dans nos expériences sur les plantes bulbeuses.

Pour un bulbe se trouvant sur la limite de la puissance de floraison et qui a dans l'été 3 à 4 jeunes feuilles (feuilles destinées à l'assimilation l'année suivante), deux alternatives se produisent après qu'il a été planté en octobre : a) *Il ne fleurit pas* : les petites feuilles, présentes en été, donneront 3 ou 4 feuilles, toutes à la base ; — les feuilles formées après l'été, naissantes au même point végétatif terminal, donneront un nouveau bulbe à l'intérieur de l'ancien ; — ce nouveau bulbe sera enveloppé et protégé plus tard par les 2 à 4 bases membraneuses des feuilles desséchées. b) *Il fleurit* : des feuilles de l'été précédent, *une* seulement reste à la base, portant à son aisselle un bourgeon qui va donner naissance à un bulbe latéral beaucoup plus gros que dans le cas (a) ; — entre les feuillettes se développent des entre-noeuds ; — toutes ces feuilles, à partir de la deuxième, tant celles présentes en été que celles formées en hiver, se trouveront insérées dans une tige qui doit encore s'allonger ; — les deux dernières, sans entre-noeuds, devenant vertes et assez grandes, forment la spathe de l'inflorescence. Celle-ci se forme, chez *l'Imperator*, fin février-mars et se compose d'une première fleur, puis d'une seconde à l'aisselle de

la deuxième feuille de la spathe (Cf. la publication suivante sur le développement périodique des Iris bulbeux). Mais dès la fin d'octobre, le traitement de l'été a décidé de la possibilité que le bulbe suivra. Sous 3 à 8, on trouve les tableaux de bulbes de diverses mesures, les températures auxquelles ils ont été soumis, ainsi que le pourcentage de ceux qui fleurissent. Après 23° en été, les bulbes-limite produiront l'année suivante le plus haut pourcentage de fleurs; en ce qui concerne les plus gros bulbes, le souci de la plante future doit nous faire préférer une température de 17 à 20°. Les bulbes trop petits qui, même après 23°, donneront trop de plantes sans fleurs, ne se vendent pas. Ceux-ci auraient dû être traités de manière à fleurir le moins possible chez le cultivateur et à donner de nouveaux bulbes arrondis et assez gros pour fleurir une année plus tard sans difficultés après un traitement à 17—20°. (Les bulbes latéraux aplatis dans l'aisselle des feuilles à la base d'une plante ayant fleuri ont beaucoup plus de défauts pour le commerce que les bulbes ronds terminaux du même poids. — Cf. GRIFFITHS 1928, notre texte hollandais, et les prochaines publications). La marche à suivre pour obtenir des plantes ne fleurissant pas est donnée partiellement dans les Tabl. aux températures de 9 (et de 5) degrés. Mais ici, nous ne donnons pas encore d'instructions exactes: il nous reste à savoir si ces températures basses pourront faire grandir les bulbes dans une mesure satisfaisante. Nous reviendrons sur ce sujet dans les prochaines publications; d'autres expériences sont en cours. Nous espérons surtout rassembler des données sur le mode de réaction dans l'état remarquable où les organes de la plante se trouvent *sur la limite de l'aptitude à former fleurs.*

Mathematics. — *Electromagnetism, independent of metrical geometry. 3. Mass and motion.* By D. VAN DANTZIG. (Communicated by Prof. J. A. SCHOUTEN).

(Communicated at the meeting of October 27, 1934).

§ 1. *Introduction.*

In two previous papers [1]) I have shown that MAXWELL's equations may be brought into a form, which is independent of metrical geometry. In this paper I will show that the same can be done for the equations which determine the motion of a particle in an electromagnetic field. In the ordinary (relativistic) theory this is not the case, as $k_i\, ds = e/c \,.\, F_{ij}\, d\xi^j$ is proportional with the *covariant* differential belonging to the RIEMANNian

[1]) D. VAN DANTZIG Electromagnetism, independent of metrical geometry. 1. The foundations; 2. Variational principles and further generalisation of the theory, these Proceedings **37** (1934) 521—525; 526—531, abbreviated as EM 1, 2

connection, determined by g_{ij}, of the velocity $i_j = g_{jh}\, d\xi^h/ds$, which itself also depends on the g_{ij}.

To this end I give in § 2 a new form of the equations of dynamics, which is based on the LAGRANGian theory of *classical* dynamics and which might be of interest in itself. In § 3 it is shown that the vector of total momentum and energy is constant in the sense of LIE-derivation. § 4 contains the proof that also the equations of motion of *relativity* mechanics are a particular case of the general theory of § 2. It is remarkable that the *only* difference between classical and special relativistic dynamics results in the different values of the kinetic energy. In § 5 the mass-charge-relation of EDDINGTON is deduced by a very simple and evident identification which leads at once to the physical meaning of the φ_i in the interior of a particle, viz. c/e times the total momentum and energy. Finally in § 6 the relations between the mass and charge-density are studied in somewhat greater detail. It is shown that the total mass may be considered as an electromagnetic interaction-energy; and the equation of motion is interpreted in such a way that in a certain sense the well-known difficulty of the "non-MAXWELLian forces", which should keep together a particle, disappears.

The unification of electromagnetism with classical and relativistic dynamics is of a very intrinsic nature, and not only a formal one. It seems to be about the most intimate one [1]) which can be reached without introducing quantum-mechanics.

§ 2. *A new form of the dynamical equations.*

Let us consider a dynamical system with n degrees of freedom, determined by n independent dynamical coordinates q^α ($\alpha, \beta, \gamma, \delta, \varepsilon = 1, \ldots, n$). We will suppose the system to be holonomic and derivable from a LAGRANGian function

$$L = T - P, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where both T and P are allowed to depend on the q^α, the $\dot{q}^\alpha$ and t. We put $q^0 = t$, and suppose the indices $\varkappa, \ldots, \tau$ to run over the values $0, 1, \ldots, n$. The differential

$$d\Lambda = L\, dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

is homogeneous of degree one (but not linear) in the $n+1$ differentials dq^λ. This is the *only* property of $d\Lambda$ we make use of. We put

$$j_\lambda = \partial T\, dt/\partial\, dq^\lambda \;, \quad f_\lambda = -\partial P\, dt/\partial\, dq^\lambda \;, \quad p_\lambda = \partial L\, dt/\partial\, dq^\lambda = j_\lambda + f_\lambda \,. \quad (3)$$

[1]) Apart from *formal* simplifications, obtainable by the use of a five-dimensional formalism.

Hence the $p_\lambda, j_\lambda, f_\lambda$ are *homogeneous of degree zero in the differentials.* Especially if the mechanism is conservative $f_\alpha = 0, -f_0 = P =$ potential energy, $-j_0 = T =$ kinetic energy, $-p_0 = H =$ total energy, $p_\alpha = j_\alpha =$ momentum. However, in the general case also we will call $j_\lambda, f_\lambda, p_\lambda$ the kinetic, the potential and the total vector of momentum and energy. From EULER's theorem about homogeneous functions follows

$$d\Lambda = p_\lambda\, dq^\lambda = p_\alpha\, dq^\alpha - H\, dt \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The equations of motion, found by variation of $\int d\Lambda$ are

$$d\,\frac{\partial\, d\Lambda}{\partial\, dq^\lambda} - \frac{\partial\, d\Lambda}{\partial\, q^\lambda} = 0\,^{1}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

or

$$dp_\lambda - \partial\, d\Lambda/\partial\, q^\lambda = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Now let us consider n independent first integrals which can be solved for the $\dot{q}^\alpha$: $\dot{q}^\alpha = \dot{q}^\alpha\,(q^\alpha, t, c_1, \ldots, c_n)$. For each system of constants we have in the $(n+1)$-dimensional space X_{n+1} of the q^λ a field of *directions*, the ratio's of the dq^λ being functions of the q^λ. If we divide the dq^λ by an arbitrary differential $d\tau$ [2]), the $dq^\lambda/d\tau$ are functions of the q^λ. As the differential $d\tau$ disappears automatically from all equations, we will omit it altogether and write simply

$$dq^\lambda = dq^\lambda\,(q^\mu) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Then also the p_λ are functions of the q^λ. It is well known that the equations can be integrated by quadratures if the first integrals are in involution, viz. if

$$\partial_{[\mu}\, p_{\lambda]} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Now let us consider (7) as defining n *unknown* functions of the q^λ, which we will substitute in p_λ and in the equation of motion (6). The first term in (6) becomes simply

$$dp_\lambda = (\partial_\mu\, p_\lambda)\, dq^\mu, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where

$$\partial_\mu = \frac{\partial}{\partial q^\mu} + \frac{\partial\, dq^\rho}{\partial\, q^\mu} \cdot \frac{\partial}{\partial\, dq^\rho}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

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[1]) Cf. for this symmetrical form of LAGRANGE's equation P. A. M. DIRAC, Homogeneous variables in classical dynamics. Proc. Cambridge Philosophical Society **29** (1933) 389-400.

[2]) A special choice of $d\tau$, which may sometimes be of use (Cf § 3), is $d\tau = d\Lambda$.

so that $\partial_\mu p_\lambda$ is the "total partial derivative" of p_λ, whereas $\partial p_\lambda / \partial q_\mu$ is only the "partial partial derivative", i.e. the partial derivative under the condition $dq^\lambda = \text{const.}$

The second term in (6) becomes by using (10):

$$\frac{\partial\, d\Lambda}{\partial q^\lambda} = \frac{\partial\, p_\mu\, dq^\mu}{\partial q^\lambda} = \frac{\partial\, p_\mu}{\partial\, q^\lambda}\, dq^\mu = (\partial_\lambda p_\mu)\, dq^\mu - \left(\frac{\partial\, dq^\rho}{\partial q^\lambda} \cdot \frac{\partial\, p_\mu}{\partial\, dq^\rho}\right) dq^\mu. \quad . \quad (11)$$

But the last term in (11) is zero, because

$$\frac{\partial\, p_\mu}{\partial\, dq^\rho}\, dq^\mu = \frac{\partial^2\, d\Lambda}{\partial\, dq^\rho\ \partial\, dq^\mu}\, dq^\mu = \frac{\partial\, p_\rho}{\partial\, dq^\mu}\, dq^\mu = 0, \quad . \quad . \quad . \quad (12)$$

p_μ being homogeneous of degree zero in the dq^μ (EULER's condition). Hence (11) becomes

$$\partial\, d\Lambda / \partial\, q^\lambda = (\partial_\lambda\, p_\mu)\, dq^\mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and the equation of motion (6) is equivalent with

$$\boxed{2\, dq^\mu\, \partial_{[\mu}\, p_{\lambda]} = 0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

In (14) the p_λ are *given* functions of the q^λ, dq^λ, determined by (3), and the dq^λ are *unknown* functions of the q^λ, so that (14) is a set of $n+1$ partial differential equations of the first order for the n ratio's of the $n+1$ unknown functions dq^λ (q^μ). Indeed the $(n+1)$ equations (14) are dependent, the left side of (14) becoming zero by transvection with dq^λ. Evidently each solution of (8) satisfies (14), the operator ∂_μ, used in (8) i.e. $\partial / \partial q_\mu$ *after* first integration, being equivalent with the operator (10) *before* integration. But a solution of (14) will in general *not* satisfy (8).

§ 3. LIE-*derivative and energy-law.*

The equations of motion (14) admit of a very simple interpretation with the aid of the LIE-derivative, introduced by ŚLEBODZIŃSKI [1]), and studied in great detail by SCHOUTEN and VAN KAMPEN [2]).

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[1]) W. ŚLEBODZIŃSKI, Sur les équations canoniques de Hamilton, Bull. Ac. Royale de Belgique (5) **17** (1931) 864—870.

[2]) J. A. SCHOUTEN & E. R. VAN KAMPEN, Beiträge zur Theorie der Deformation, Warszawa Prac Mat.-Fiz. **41** (1933) 1—19. The name „LIE-derivative" was proposed by D. VAN DANTZIG, Zur allgemeinen projektiven Differentialgeometrie. II. X_{n+1} mit eingliedriger Gruppe, Proc. Kon. Ak. **35** (1932) 535—542.

We consider an infinitesimal point-transformation $q^\lambda \to q^\lambda + dq^\lambda$, determined by a vectorfield $v^\lambda = \varepsilon^{-1}\, dq^\lambda$. Under this transformation any object $X^{\cdot\cdot}_{\cdot\cdot}$ [1]), defined in a point q^λ can be "dragged along", i. e. we can consider the values q^λ as new coordinates of the point $q^\lambda + \varepsilon v^\lambda$ and consider the object $Y^{\cdot\cdot}_{\cdot\cdot}$ whose components in this *second* point with respect to these *new* coordinates are equal to the components of $X^{\cdot\cdot}_{\cdot\cdot}$ in the *first* point with respect to the *old* coordinates. The difference of $Y^{\cdot\cdot}_{\cdot\cdot}$ and the value $X^{\cdot\cdot}_{\cdot\cdot} + dX^{\cdot\cdot}_{\cdot\cdot}$ of $X^{\cdot\cdot}_{\cdot\cdot}$ in the point $(q^\lambda + dq^\lambda)$ divided by ε will be called the LIE-derivative of $X^{\cdot\cdot}_{\cdot\cdot}$ and denoted by $\underset{L}{D} X^{\cdot\cdot}_{\cdot\cdot}$. It determines a mode of *invariant* differentiation, which is independent, not only of the choice of arbitrary coordinates, but also of *any connection*. It follows the ordinary rules for derivation of sums, products and transvections. As examples we mention:

contravariant vector: $\underset{L}{D}\, u^\lambda = v^\mu\, \partial_\mu\, u^\lambda - u^\mu\, d_\mu\, v^\lambda$

density of weight $\mathfrak{k}$: $\underset{L}{D}\, \mathfrak{p} = \mathfrak{k}\mathfrak{p}^{(\mathfrak{k}-1)/\mathfrak{k}}\, \partial_\mu\, (v^\mu\, \mathfrak{p}^{1/\mathfrak{k}})$

covariant vector: $\underset{L}{D}\, w_\lambda = v^\mu\, \partial_\mu\, w_\lambda + w_\mu\, \partial_\lambda\, v^\mu =$

$$= 2\, v^\mu\, \partial_{[\mu}\, w_{\lambda]} + \partial_\lambda\, (w_\mu\, v^\mu).$$

From the last example we see that especially

$$\underset{L}{D}\, w_\lambda = 2\, v^\mu\, \partial_{[\mu}\, w_{\lambda]} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

if v^λ satisfies the condition $w_\mu\, v^\mu = \text{const}$.

Now the vectorfield v^λ is *not* uniquely determined by the infinitesimal displacement dq^λ. But, if dq^λ is the deplacement belonging to a mechanical system as considered in § 2, we can determine it uniquely by requiring

$$p_\lambda\, v^\lambda = 1\ ^{2}), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

viz. by putting

$$v^\lambda = dq^\lambda / d\Lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

From (15), (16) we see that the equations of motion (14) can be written in the very simple form [3])

$$\boxed{\underset{L}{D}\, p_\lambda = 0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

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[1]) The big points stand for arbitrary rows of indices.

[2]) Comp. note [2]) on page 645 and equation (4).

[3]) Equation (18) is equivalent with the following fact: if an arbitrary line-element $\underset{1}{dq^\lambda}$ is "dragged along" by the transformation (i.e. if $\underset{L}{D}\, \underset{1}{dq^\lambda} = 0$), $p_\lambda\, \underset{1}{dq^\lambda}$ is constant in the ordinary sense. Another equivalent form of this theorem was already found by HAMILTON (III. Supplement to the Theory of systems of rays). C.f. M. HERZBERGER Strahlenoptik, Berlin, J. SPRINGER (1931), p. 12.

and express the fact that the *vector of total momentum and energy is constant in the sense of the* LIE*-derivative during the motion*, under the assumption that v^λ be determined by (17) [1]).

§ 4. *Relativistic motion in an electromagnetic field.*

In this paragraph we will apply the general results of § 2 to the motion of a *material point*. Hence we will write ξ^a $(a, \ldots, g = 1, 2, 3)$ instead of q^α, $\xi^4 = ct$ instead of $q^0 = t$, and use the indices $h, i, \ldots, m = 1, 2, 3, 4$ instead of $\iota, \varkappa, \ldots, \tau = 0, 1, \ldots, n$.

If we split up p_i according to (3) into its kinetic and potential part:

$$p_i = j_i + f_i, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

the equation of motion (14) can be written:

$$2\, d\xi^j\, \partial_{[j}\, j_{i]} = -\,2\, d\xi^j\, \partial_{[j}\, f_{i]} \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

As this equation is valid for *any* form of the LAGRANGian, we can apply it in particular to the *relativistic* motion (of course by using the fundamental tensor!). To this end we put:

$$T\, dt = -\, m\, c\, ds, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

where m is the *invariant* mass of relativity and

$$ds = \sqrt{g_{ij}\, d\xi^i\, d\xi^j} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

is the RIEMANNian line-element of general relativity (signature $- - - +$). Then from (3) follows:

$$j_i = -\, m\, c\, g_{ih}\, i^h, \quad i^h = d\xi^h/ds, \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

in particular $j_4 = -\, mc\,(1 - v^2/c^2)^{-1/2}$. Instead of the relation $cj_4 = -\, T$, which holds only in classical dynamics, we have here $cj_4 = T/(1 - v^2/c^2)$, whereas the j_a have the same values as in classical dynamics. Now equation (20) can be written with *covariant* derivatives $\nabla_j\, j_i = \partial_j\, j_i - \Gamma^h_{ji}\, j_h$, the RIEMANNian connection being symmetrical, so that $\nabla_{[j}\, j_{i]} = \partial_{[j}\, j_{i]}$. Hence the left side of (20) is equal to

$$-\, 2\, mc\, d\xi^j\, \nabla_{[j}\, i_{i]} = -\, mc\, ds\, i^j\, (\nabla_j\, i_i - \nabla_i\, i_j) \quad . \quad . \quad . \quad (24)$$

But, the last term in (24) is zero because of $i^j\, \nabla_i\, i_j = \tfrac{1}{2}\, \nabla_i\, i^j\, i_j = 0$, $i^j\, i_j$ being constant $(= 1)$. Hence the second member of (24) is equal to

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[1]) Moreover this is the *only* determination (but for a constant factor) for which the theorem holds.

$-mc\,\delta\,i_i$, where $\delta\,i_i = d\xi^j\,\nabla_j\,i_i = ds\,i^j\,\nabla_j\,i_i$ is the *covariant differential* of i_i, and we can write equation (20) in the well-known form

$$\delta\,j_i/ds = k_i\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

where k_i is the relativistic force-vector. It is equal to

$$k_i = (2\,\partial_{[i}\,f_{j]})\,d\xi^j/ds, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

which expression is equally well known in relativity: indeed, putting

$$f_i = e/c\,.\,\varphi_i\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

so that the action-differential is

$$d\,\Lambda = -mc\,ds + e/c\,.\,\varphi_i\,d\xi^i\,, \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

(26) becomes:

$$k_i = e/c\,.\,F_{ij}\,d\xi^j/ds, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

which is the relativistic LORENTZ-force, working on a charge e. Hence we see that the form (29) or (26) of the force is not at all particularly special but is the most general one which is derivable from a kinetic potential. The only specialisation lies in the fact that φ_i *does not depend on the velocity.*

§ 5. *Mass, and the physical meaning of the* φ_i.

Now let us suppose that the φ_i may be expressed by means of the retardated potentials of special relativity, belonging to empty space:

$$\varphi_i\,(P) = \int g_{ij'}\,\gamma\,(P, P')\,\mathfrak{z}^{j'}\,(P')\,d\,\Sigma'\,, \quad . \quad . \quad . \quad . \quad . \quad (30)$$

where $\gamma\,(P, P')$ is the special operator, introduced in EM 2 (14):

$$\gamma\,(P, P') = -1/4\pi r\,.\,\delta\,(ct - ct' - r\,(P, P'))\,, \quad . \quad . \quad . \quad (31)$$

and that only a *finite* number of particles have an appreciable influence on the field. Then the integral (30) may be replaced by a finite sum over all particles P':

$$\varphi_i = \sum_{P'} \overset{P'}{\varphi}_i\,(P) \quad , \quad \overset{P'}{\varphi}_i\,(P) = -e'/4\pi r\,.\,g_{ih'}\,i^{h'}\,, \quad . \quad . \quad . \quad . \quad (32)$$

the $i^{j'}$ being taken at the time $t' = t - r/c$. Now the analogy between the right sides of equations (32) and (23) is very striking. Hence it seems natural to suppose that j_i, or rather (corresponding to (27))

$$\overset{0}{\varphi}_i = c/e\,.\,j_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

is the mean value of a covariant vector $\overline{\overset{0}{\varphi}}_i$, which is of quite the same nature as the other φ_i, i.e. that $c/e\,.\,j_i$ is a *mean value of the potential-vector* $\overset{0}{\varphi}_i$ *of the particle P itself, taken over its own volume.* Then the

r in the denominator of (32), will have to be replaced by some mean value a, and we get $m \,.\, c/e = e/4\pi ca$, or

$$mc^2 = e^2/4\pi a.\ ^{1}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

Equation (34) was first deduced by EDDINGTON [2]) from a hypothesis which is equivalent with (33).

We may now write

$$c/e \,.\, p_i = \varphi_i = \overset{u}{\varphi}_i + \overset{0}{\varphi}_i, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

φ_i, $\overset{u}{\varphi}_i$ and $\overset{0}{\varphi}_i$ being *the total, the exterior and the interior potential vector*, and we can say that their physical meaning is that *they are equal to e/c times the total, the potential and the kinetic momentum and energy* respectively. If we substitute (35) in (14), bring the factor e/c *before* the differential symbol and divide by $d\Sigma$, the equation of motion becomes: [3])

$$\boxed{\mathfrak{s}^j F_{ji} = 0}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

where again $F_{ji} = 2\,\partial_{[j}\,\varphi_{i]}$ is the *total* (exterior and interior) field. Though the first factor $\mathfrak{s}^j$ in (36) belongs only to the particle itself, it may be identified with the *total* current, the $\mathfrak{s}^i$ of the exterior field being zero on the place where the particle is.

It follows from (36) that for $\mathfrak{s}^k \neq 0$ the total bivector F_{ij} is simple:

$$F_{[ij}\,F_{kl]} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

Hence it is geometrically represented by a (local) twodimensional cylindrical tube, envelopped by ∞^2 planes. Equation (36) expresses the fact that *the directions of the total current is always contained in the 2-direction of the tube, belonging to the total field-bivector.*

EDDINGTON deduces l.c. equation (34) from the hypothesis that a particle moves always in such a way that the total field F_{ij} is zero. This condition which is equivalent with equation (8) is, however, to strong, as it contains *six* independent equations for the three unknown ratio's of the $d\xi^i$, whereas our general equation (36) contains only *three* independent conditions. It is, however, easily seen, that EDDINGTON for his deduction only makes use of the weaker condition (36). Nevertheless it might be that EDDINGTON's condition $F_{ij} = 0$ were characteristic for *special kinds* of particles [4]), in the same way as *photons* are characterised by another trivial specialisation of (36), viz. $\mathfrak{s}^i = 0$, $F_{ij} \neq 0$.

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[1]) We find for an electron with $m = 0.9 \times 10^{-27}$ g, $e = -\,4.77 \times 10^{-10}\,\sqrt{4\pi}$ $g^{1/2}\,\mathrm{cm}^{3/2}\,\mathrm{sec}^{-1}$ and $c = 3 \times 10^{10}$ cm sec^{-1}, $a = 2.8 \times 10^{-13}$ cm, which is about the radius of an electron, as it is usually supposed to be.

[2]) Sir A. S. EDDINGTON, The mathematical theory of relativity, Cambridge University Press (1923), § 80, Explanation of the mechanical force. EDDINGTON's explanation has been greatly inspiring to this part of my investigations.

[3]) $\mathfrak{s}^i$ is the current-density. Hence if $\varrho = \mathfrak{s}^4$ is the charge-density, $\mathfrak{s}^i d\Sigma = \varrho/c \,.\, d\xi^i/dt \,.\, d\Sigma = \varrho\, d\mathfrak{V}/c \,.\, d\xi^i$, which becomes $e/c \,.\, d\xi^i$ after integration over the volume of the particle.

[4]) Probably these would be particles without spin.

§ 6. *Mass as interaction-energy.*

The formula for the mass of a charged particle may be brought into another and more expressive form. Such a particle is a set of ∞^3 paths in space-time, filling up the interior of a threedimensional tube. Its charge is

$$e = c \int^3 \mathfrak{s}^i \, d\mathfrak{S}_i \,, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

integrated over any threedimensional space X_3, which cuts each streamline once. It follows from the equation of continuity $\partial_i \mathfrak{s}^i = 0$ that e is the same for each such X_3.

Now the HAMILTONian, belonging to the action

$$\mathfrak{W} = \tfrac{1}{4} F_{ij} \mathfrak{H}^{ij} - \tfrac{1}{2} \varphi_i \mathfrak{s}^i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

(comp. EM 2 (7)) is $\mathfrak{H}$ if

$$\left. \begin{aligned} -\mathfrak{H} = \mathfrak{W} - \varphi_i \frac{\partial \mathfrak{W}}{\partial \varphi_i} = \tfrac{1}{2} \mathfrak{H}^{ij} \frac{\partial \mathfrak{W}}{\partial \mathfrak{H}^{ij}} - \mathfrak{W} = \tfrac{1}{4} F_{ij} \mathfrak{H}^{ij} + \tfrac{1}{2} \varphi_i \mathfrak{s}^i = \\ = \varphi_i \mathfrak{s}^i + \tfrac{1}{2} \partial_i (\mathfrak{H}^{ij} \varphi_j). \end{aligned} \right\} \quad . \quad (40)$$

Hence, if the boundary-integral vanishes:

$$H = \int^4 \mathfrak{H} \, d\Sigma = - \int^4 \varphi_i \mathfrak{s}^i \, d\Sigma \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

We will suppose the *exterior* field to be zero: $\varphi_i = \overset{0}{\varphi}_i$ and we will extend the integral (41) over some finite length of the tube. If this is filled up arbitrarily with $\infty^1 X_3$, each cutting each streamline once, we may decompose each fourdimensional volume-element $d\Sigma$ into a threedimensional volume-element $d\mathfrak{S}_i$ of the X_3 passing through it and a line-element $d\xi^i$, in the direction of $\mathfrak{s}^i$:

$$d\xi^i = \mathfrak{s}^i \, d\mathfrak{X}, \quad d\Sigma = d\xi^i \, d\mathfrak{S}_i \,. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

Then (41) becomes:

$$-H = \int \varphi_i \mathfrak{s}^i \mathfrak{s}^j \, d\mathfrak{X} d\mathfrak{S}_j = e/c \,. \int \bar{\varphi}_i \, d\xi^i \,, \quad . \quad . \quad . \quad . \quad (43)$$

where $\bar{\varphi}_i$ is a mean-value of φ_i over the X_3 in the interior of the tube. *Now we suppose* $\bar{\varphi}_i$ *to be identical with the* $\overset{0}{\varphi}_i$ *of (33).* Then (43) becomes

$$H = - \int j_i \, d\xi^i = \int mc \, ds \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

Comparing (44) with (41) we find:

$$m \, ds/dt \overset{*}{=} - \int \varphi_i \mathfrak{s}^i \, d\mathfrak{S}_4 \overset{*}{=} - \int d\mathfrak{S}_4 \int d\Sigma' \, \gamma_{ij'} \mathfrak{s}^i \mathfrak{s}^{j'} \quad . \quad . \quad (45)$$

From (44) and (41) follows also that the mass-density $\mathfrak{m}$ is determined by

$$\mathfrak{m} c = \mathfrak{H} \,. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45^a)$$

If the particle is in rest with respect to a GALILEian frame (hence $\mathfrak{S}^a \overset{*}{=} 0$), and has spherical symmetry, its charge-density being given by $\mathfrak{S}^4 \overset{*}{=} \varrho = \varrho(r)$, its potential is

$$\varphi(r) \overset{*}{=} \frac{1}{r}\int_0^r \varrho(r')\, r'^2\, dr' + \int_r^\infty \varrho(r')\, r'\, dr', \quad . \quad . \quad . \quad . \quad . \quad (46)$$

provided that we may take the ordinary COULOMB-potential. Its charge and mass are according to (38) and (45):

$$e \overset{*}{=} 4\pi \int_0^\infty \varrho(r)\, r^2\, dr, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (47)$$

$$mc \overset{*}{=} 4\pi \int_0^\infty \varrho(r)\, \varphi(r)\, r^2\, dr.\ ^1) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

From the fact that $\varrho(r)$ occurs (after substitution of (46) in (48)) in a *quadratic* way in m, it follows that the invariant mass of a *system* of particles is *not* equal to the sum of the invariant masses, even if the particles are in rest relative to each other. However, if the mutual distances of the particles are large compared with each a, the cross-terms of the form $\overset{1}{\varrho}\,\overset{2}{\varphi}\, d\mathfrak{S}_4$ will be small, $\overset{2}{\varphi}$ being small when $\overset{1}{\varrho}$ is great and vice versa, so that in that case the invariant mass is at least *approximately* additive.

As another consequence from the fact that ϱ occurs quadratically in m, we remark that *to a given value of m belong two particles of equal but opposite charges.*

Equation (45) expresses the fact that *the total mass of a particle is of the kind of an electromagnetic interaction-energy*. From this point of view the equation of motion (36) may be interpreted as follows: *a charged particle moves as if it were composed of ∞^3 point-charges, leading to a resulting current-density $\mathfrak{S}^i$, each of which has a mass zero, and moves in such a way, that the total* LORENTZ*-force which works on it is zero* [2]). Hence in a certain sense each point-charge is a *cause* of the electromagnetic field, but is not *influenced* by it, so that no non-MAXWELLian forces are needed to keep them together.

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[1]) If we take as an example $\varrho = \varrho_0\, e^{-r/b}$ where b is some given length, we find $e = 8\pi\varrho_0 b^3$, $\varphi = e/4\pi r\{1-(1+r/2b)e^{-r/b}\} \sim e/4\pi r$ for $r \gg b$, $mc = 5e^2/64\pi b$ and $a = {}^{16}/_5\, b$. With this density, however, a much to great part of the charge would ly outside of a sphere of radius a; a density as e.g. $\varrho = \varrho_0\, e^{-r^2/b^2}$ would fit much better. Ir. L. KOSTEN attires my attention to the fact that a pure surface-charge [i.e. $\varrho(r) = \varrho_0\, \sigma(r-b)$] leads exactly to $a = b$.

[2]) Mathematically this means that the splitting up of dA into $L\,dt$ and $-P\,dt$, which is rather arbitrary (because the secondterm also depends on the differentials), is effectuated by taking $T = 0$ and $L = -P$. This is trivial (and can be done for any mechanism), but the possibility of interpreting mass as interaction-energy (which depends on (30)) is not.

Palaeontology. — *Die Spezialisation des Incisivengebisses bei den javanischen Hippopotamidae.* Von G. H. R. VON KOENIGSWALD. (Communicated by Prof. L. RUTTEN.)

(Communicated at the meeting of October 27, 1934).

Die erfolgreichen Aufsammlungen von fossilen Vertebraten durch die „Javakaarteering van den Opsporingsdienst (Dienst van den Mijnbouw)" haben zahlreiche Reste auch von *Hippopotamus* geliefert, die zeigen, dass diese Gattung im Pliocän und Pleistocän von Java durch mehrere gut unterschiedene Arten vertreten ist, welche nicht allein brauchbare Leitfossilien darstellen, sondern und überdies neue Einblicke in die Entwickelung dieser Tiergruppe erlauben. Eine Bearbeitung des wichtigen Materiales durch den Verf. ist. z.Z. im Gange. An dieser Stelle soll nur ein Problem behandelt werden, das auch zum Verständnis der lebenden *Hippopotamus*-Arten von besonderer Bedeutung ist: *die Spezialisation des Incisivengebisses.*

Das rezente afrikanische *Hippopotamus amphibius* L. (mit zahlreichen Lokalformen) is *tetraprotodont*, d.h. es besitzt im Unter- wie im Oberkiefer in jeder Hälfte nur noch je zwei Incisiven. Welcher Incisiv nun durch völlige Reduktion unterdrückt worden ist, darüber bestehen verschiedene Auffassungen. Meist wird angenommen, dass sowohl unten wie oben der äusserste Incisiv (I_3^3) verloren gegangen sei. Begründet wird diese Auffassung einmal durch Gebissanomalien an recentem Material — bei überzählig auftretenden Incisiven pflegt der äusserste der schwächste zu sein —, ferner glaubt REYNOLDS beim eben geborenen Tiere noch die Anlage eines dritten Incisiven im Unterkiefer nachweisen zu können. Bei einem von ihm abgebildeten Unterkiefer eines Neonatus erkennt man zwischen dem zweiten Incisiven (DI_2) und dem Eckzahn (DC) in der Tiefe einer Alveole nahe dem letzteren einen winzigen Zahnkeim, den er als DI_3 deutet, eine Auffassung, der sich auch M. WEBER in seinem Handbuch „Die Säugetiere" (Bd. II, pg. 546) angeschlossen hat. W. O. DIETRICH jedoch bezweifelt die Richtigkeit der REYNOLDS'schen Deutung und erklärt überzeugender den in Frage stehenden Zahn für den Keim des bleibenden Eckzahns (C); im übrigen hält auch er an der Auffassung fest, dass bei *H. amphibius* die I_3^3 unterdrückt seien.

Eine andere Meinung vertritt LYDEKKER. Ausgehend von den Unterkiefern des indischen *H. namadicus* FALC. mit stark und des *H. palaeindicus* FALC. mit noch stärker reduziertem mittleren Incisiven (I_2) glaubt er schliessen zu dürfen, dass auch dieser Zahn bei *amphibius* der geschwundene sei. Vom oberen Incisivengebiss der indischen Arten lagen

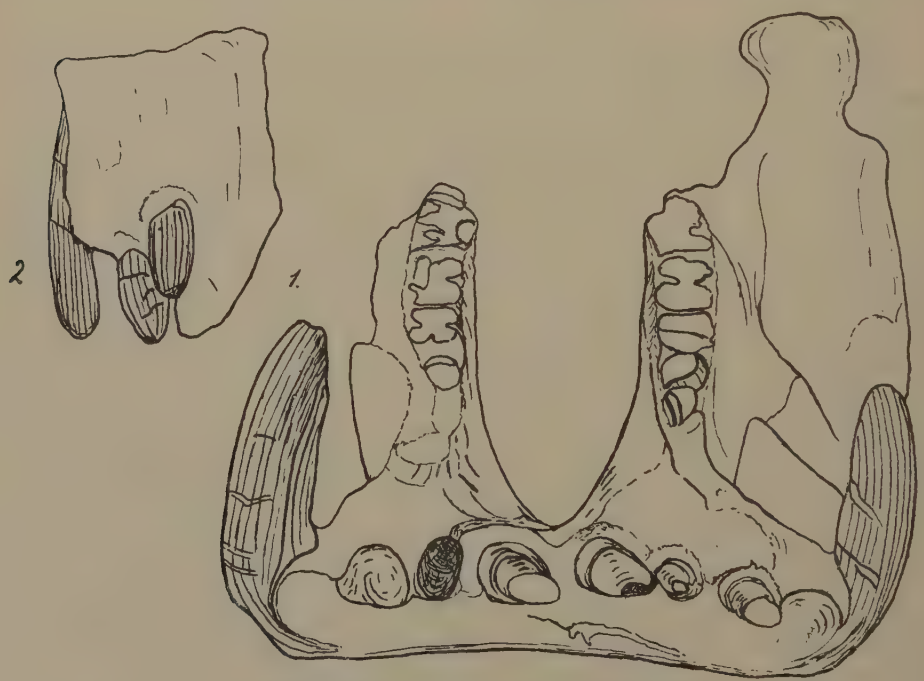
ihm keine Reste vor, er vermutet aber, dass auch hier und entsprechend bei *H. amphibius* der mittelste Incisiv der unterdrückte wäre. Seine Ansicht wird aber, soweit ich die Literatur übersehe, von fast niemandem geteilt, von DIETRICH sogar heftig angegriffen, obwohl die Richtigkeit seiner Auffassung für die Unterkiefer der *asiatischen* Formen zweifellos zutrifft; im Oberkiefer liegen die Verhältnisse dagegen, wie wir noch sehen werden, etwas anders.

Die javanischen Funde ergeben hier neue Gesichtspunkte. Das Incisivengebiss der verschiedenen Arten — die alle, wie auch die indischen Formen, sechs Schneidezähne aufweisen, d.h. *hexaprotodont* sind — weist die folgenden Eigenheiten auf:

Hippopotamus simplex V. KOENIGSWALD.

Unterkiefer: Die Austrittsstellen der unter sich etwa gleich starken Incisiven sind in einer Reihe angeordnet. Bei dem hier abgebildeten Kiefer ist I_3 etwas stärker, I_2 etwas schwächer als I_1.

Oberkiefer: Die drei Incisiven weisen in dem einzigen bekanntgewordenen Schnauzenstück keine besonderen Grössenunterschiede auf.



Hippopotamus simplex V. KOENIGSWALD.

Abb. 1. Unterkiefer mit I_1 rechts und $I_{1—3}$ links. Von I_2 und I_3 rechts sind nur die Alveolen vorhanden. Breite zwischen den Eckzähnen 24.5 cm.

Abb. 2. Rechtes Praemaxillare mit $I^{1—3}$. Grösste Länge 10.8 cm. Beide Stücke von K. Glagah, Boemiajoe: (Unterer Wirbeltierhorizont).

Hippopotamus antiquus V. KOENIGSWALD.

Unterkiefer: Die Incisiven sind alle von gleicher Grösse, doch liegen die Austrittsstellen der I_2 höher als für die daneben stehenden Zähne.

Oberkiefer: Ein Schnauzenfragment lässt erkennen, dass I^3 nur etwa halb so gross ist wie I^2, der vermutlich von I^1 in der Grösse noch wenig übertroffen wurde.

Hippopotamus antiquus V. KOENIGSWALD.

Abb. 3. Unterkieferfragment mit den Austrittsstellen des rechten *C* und der sechs gleich grossen *I*, alle abgebrochen. Grösste Breite der Schnauze 22.1 cm.

Abb. 4. Schnauzenpartie. Rechts ein sehr abgebrauchter P^2, P^1 abgebrochen, der Eckzahn ausgefallen. Von I^3 erkennt man die untiefe, oben geschlossene kleine, von I^2 die grössere, unten offene (beschädigte) Alveole. Von I^1 rechts wie von I^2 links ist nur noch ein Teil der unteren Begrenzung des Alveolenrandes erhalten (*a* und *b*). I^3 links ist in der Alveole abgebrochen. Zwischen diesen und der Alveole des ausgefallenen Eckzahnes ein zufällig bei der Fossilisierung dort festzementierter Zahn (*c*). Es folgen die Reste von P^1 und P^2. Grösste Breite des Fragmentes 24.4 cm. Beide Stücke von Sidorojol nördlich Modjokerto, Ostjava.

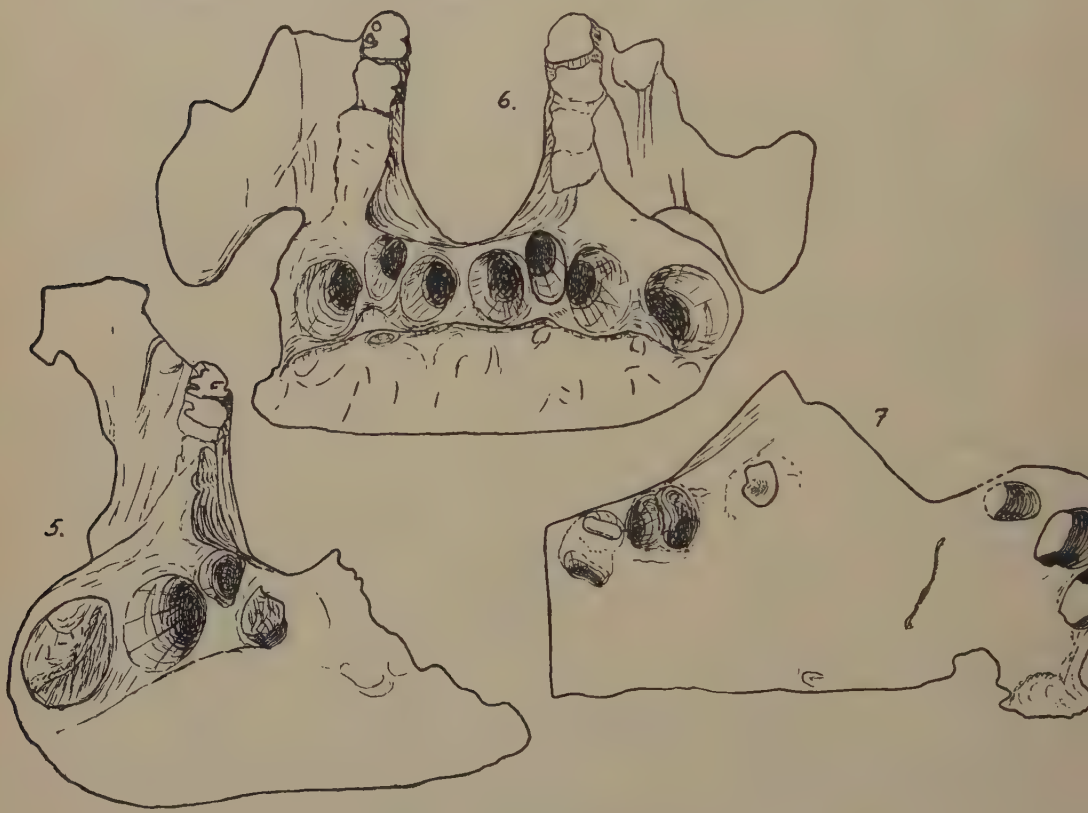
Hippopotamus namadicus FALCONER.

Dieses in den jüngeren Schichten nicht seltene *Hippopotamus* kommt so sehr mit dem aus Indien beschriebenen *H. namadicus* aus den Narbadas

überein, dass ich es damit für identisch halte (früher von mir mit *H. palaeindicus* verglichen). Für die Berechtigung einer Abtrennung als besondere Art, *H. sivajanicus* DUBOIS, kann ich keine Gründe finden.

Unterkiefer: Wie bei *H. antiquus* liegen die Austrittsstellen der aber bei dieser Art in ihrer Grösse bereits reduzierten mittleren Incisiven höher. I_1 und I_3 pflegen untereinander gleich gross zu sein. Eine Ausnahme macht ein Unterkiefer von Ngandong, bei welchem I_3 fast doppelt so gross ist wie I_1, der bei *H. amphibius* stets der grösste ist.

Oberkiefer: Aus Indien sind leider noch keine Schnauzenteile von *H.*



Hippopotamus namadicus FALCONER.

Abb. 5. Unterkieferfragment mit rechtem Ramus horizontalis. Rechts der in seiner Alveole abgebrochene C, dann die Alveole für einen auffallend grossen I_1, daneben die Alveole für den höher austretenden kleinen I_2. I_3, kleiner als I_1, ist in der Alveole abgebrochen. Die linke Kieferhälfte ist sehr beschädigt und lässt weiter keine Details erkennen. Grösste Breite des Fragments 29.5 cm.

Abb. 6. Unterkiefer mit beiden Ästen. Die Umgrenzung des rechten C ist fortgebrochen; vom linken wie von allen sechs *I* sind die Alveolen gut erkennbar, unter denen die höher liegenden der kleineren I_2 deutlich auffallen. Grösste Breite 27.6 cm.

Abb. 7. Partie des rechten vorderen Gaumens mit den Alveolen von $P^{3—1}$, C und $I^{1—3}$. Die Alveolen der drei *I* sind oben aufgebrochen. Grösste Breite des Fragments 12.2 cm. Die Originale von Abb. 5 und 6 stammen von Ngandong, das von Abb. 7 von Watoealang (Mitteljava).

namadicus bekannt geworden. Zwei javanische zeigen deutlich und übereinstimmend den in seiner Grösse reduzierten letzten Incisiven.

Ausser im Vordergebiss sind diese Arten noch durch eine ganze Reihe weiterer Merkmale unterschieden, auf die hier nicht eingegangen werden soll; die sehr verschiedene Form der Unterkiefer geht schon aus den Abbildungen hervor. *H. simplex* und *antiquus* kommen an einer Fundstelle auch nebeneinander vor und sind typisch für die älteren Ablagerungen; *namadicus* charakterisiert wie in Indien die jüngeren Schichten (Ngandong, Watoealang).

Bei *H. simplex* (wie bei dem sehr ähnlichen indischen *H. sivalensis* FALC.) zeigt das Schnauzengebiss noch keinerlei Spezialisationserscheinungen. Wohl glaubt man bei beiden Formen bereits in seltenen Fällen, — siehe die Abbildungen bei FALCONER und VAN DER MAAREL — ein beginnendes Hinaufrücken des mittleren untern Incisiven wahrnehmen zu können, doch ist dies nicht ausgesprochen und ausserdem von keiner besonderen Bedeutung für die weitere Entwicklung der asiatischen Flusspferde, da neben *H. simplex* in Boemiajoe bereits *H. antiquus* erscheint, dessen Unterkiefer nicht nur deutlicher die Verlagerung der I_2 zeigt, sondern der auch schon den abgestutzten steil abfallenden Vorderrand ausgebildet hat, den *namadicus* und *palaeindicus* gleichfalls besitzen. Bei *H. antiquus* ist I_2 gleich gross,bei *namadicus* etwa $\frac{3}{4}$, bei *palaeindicus* weniger als $\frac{1}{2}$ mal so gross wie die beiden anderen Incisiven. Wenn FALCONER *H. palaeindicus* erst als *Tetraprotodon* beschrieb, so lag das an der Dürftigkeit des ihm vorliegenden Restes. LYDEKKER hat nachgewiesen, dass diese Art hexaprotodont ist.

Da *H. antiquus* und *namadicus,* wie schon die Unterkieferform zeigt, nahe verwandt sein müssen (*H. antiquus* ist auch die geologisch ältere Form), so wird dies durch die gleichartige Veränderung beider im Oberkiefergebiss bestätigt: bei beiden ist I^3 der kleinste der Reihe.

Aus der oben gemachten Aufstellung ergibt sich somit deutlich das sehr überraschende Resultat, dass die javanischen Hippopotamidae den tetraprotodonten Zustand durch die weitergehende Reduktion ungleichwertiger Incisiven nach der Formel $\frac{I^1\ I^2}{I_1\ I_3}$ erreicht haben würden.

An sich ist die Rückbildung ungleichwertiger Incisiven, — wobei ich als Wertmesser die Nummer in der natürlichen Reihenfolge der Zähne annehme — nichts ungewöhnliches. Sie kommt bei fast allen Ordnungen der Säugetiere vor und kann zu sehr verschiedenen Resultaten führen. So hat *Tapirus* einen sehr grossen oberen bei einem fast völlig reduzierten unteren I_3. Die Chiroptera haben oben (immer) nur zwei, unten (meist) aber drei Schneidezähne; die Anthropoidea haben unten wie oben je zwei Incisiven in jeder Kieferhälfte. Bei *Galeopithecus* wird angenommen, dass I^3 oben, unten aber I_1 fehle. *Choeropsis* hat oben zwei, unten nur einen, *Dicerorhinus* oben einen, unten keinen, die Cerviden und Boviden oben keinen, unten noch alle drei Incisiven in jeder Kieferhälfte. Die Annahme

LYDEKKERs, dass bei *Hippopotamus* oben und unten stets der gleiche Incisiv unterdrückt sein müsste, hatte an sich von vorne herein nur einen geringen Grad von Wahrscheinlichkeit.

Ob wir nun berechtigt sind, die oben aus einer angestrebten Tetraprodontie der asiatischen Hippopotamidae zu folgernde Formel der Incisivenreduktion ohne weiteres auch auf die afrikanischen Vertreter dieser Gruppe zu übertragen, können wir leider nicht sicher angeben. Denn die asiatischen Formen sind niemals aus dem hexaprotodonten Stadium herausgekommen, und sie unterscheiden sich durch eine ganze Reihe weiterer Eigentümlichkeiten (Lacrymale stets getrennt vom Nasale; völlige Reduktion des P^4 individuell bei *H. namadicus* etc.) sehr wohl von den afrikanischen, die mit *H. hipponensis* GAUDRY bereits im Mittelpliocän tetraprotodont geworden sind. Die beiden Gruppen haben sich demnach sicherlich früh getrennt, vermutlich schon im Unterpliocän, und es ist a priori nicht auszuschliessen, dass sie bei der Spezialisation ihres Vordergebisses verschiedene Wege eingeschlagen haben, was an sich sehr wohl möglich ist (Beispiele oben). Wir müssen allerdings zugeben, dass der Beweis, dass es gerade die $\frac{I^3}{I_3}$ sein sollen, die bei *H. amphibius* unterdrückt werden, besonders nach der Widerlegung der REYNOLDS'schen Deutung eines noch im Milchgebiss auftretenden DI_3 durch DIETRICH in Zweifel gezogen werden kann und auf sehr schwachen Füssen steht. Auch die aus den Gebissanomalien gezogenen Schlussfolgerungen wären einmal an einer *grösseren Serie* nachzuprüfen. Wie DIETRICH hervorhebt, ist *H. amphibius* bereits als Neonatus ein *Tetraprotodon*, und man müsste versuchen durch eine Untersuchung von Foeten Klarheit in dieses interessante Problem zu bringen.

Auf die absurde Auffassung von JOLEAUD, der *Hippopotamus* von dem oberoligocänen *Aprotodon Smith-woodwardi* FORSTER COOPER der Bugti-Hügel in Belutschistan ableiten will, das, wie schon durch den Namen angedeutet ist, überhaupt keine Schneidezähne (mehr) besitzt, und der die sechs Incisiven der indischen Formen als Neuerwerbung auffasst und den ganzen Gang der Entwicklung umkehren möchte, brauchen wir hier nicht einzugehen. Eine derartige Vorstellung ist unsinnig; der Gang der Umformung des Incisivengebisses verläuft bei den Hippopotamidae im Prinzip nach den gleichen bekannten Gesetzmässigkeiten wie bei den übrigen Säugetieren. Die ersten Arten treten im Unterpliocän auf; über die praepliocäne Geschichte des Stammes sind wir sehr schlecht unterrichtet und nur auf Vermutungen angewiesen.

Zusammenfassung.

An neuem auf Java gefundenem Material fossiler Hippopotamidae wird nachgewiesen, dass die asiatischen Formen die Tendenz erkennen lassen, im Unterkiefer den mittleren (I_2), im Oberkiefer den letzten (I^3) — nicht

wie LYDEKKER vermutete, ebenfalls den mittleren — Incisiven zu reduzieren.

Ob die Reduktion des Incisivengebisses bei dem tetraprotodonten lebenden afrikanischen *Hippopotamus amphibius* L. etwa auf dem gleichen Wege erreicht worden ist — bei dieser Art wird angenommen, dass im Ober- wie im Unterkiefer $\frac{I^3}{I_3}$ verschwunden sei, wofür die gewöhnlich meist angeführten Beweise sehr schwach und z.T. widerlegt sind — ist vorläufig nicht zu beweisen und kann ohne weiteres nicht gefolgert werden, da die afrikanischen Flusspferde sich schon sehr früh von den asiatischen getrennt haben müssen, so dass nicht ausgeschlossen werden kann, dass sie bei der Reduktion des Incisivengebisses einen anderen Weg eingeschlagen haben.

Palaeontologisch Laboratorium bij den Opsporingsdienst.
(Dienst van den Mijnbouw.)

Bandoeng, Juni 1934.

Die Originale zu Abb. 1—3, 5—7 befinden sich im palaeontologischen „Museum van den Dienst van den Mijnbouw" in Bandoeng; das Original zu Abb. 4 wurde mir von Dr. J. H. COERT, Soerabaja, aus seiner Sammlung freundlichst zur Bearbeitung abgestanden, wofür ich ihm auch an dieser Stelle bestens danken möchte. — Alle Zeichnungen nach Photos vom Verfasser.

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Microbiology. — *On the metabolism of the purple sulphur bacteria.* By P. A. ROELOFSEN. (Communicated by Prof. A. J. KLUYVER).

(Communicated at the meeting of October 27, 1934).

§ 1. *Introduction.*

By the extensive and careful experiments on the metabolism of pure cultures of purple sulphur bacteria bij C. B. VAN NIEL (1931) and F. M. MULLER (1933, 1), as well as by their tempting theoretical considerations (1931), I was induced to make some kinetic studies on the metabolism of these organisms with the WARBURG manometrical method. It was my ultimate aim to determine, as has been done for *Chlorella* by O. WARBURG (1923), the energy efficiency of light of different wavelengths in the carbon dioxide assimilation of the purple sulphur bacteria (p.s.b.), but now with the special hydrogen-donators used by these bacteria.

On the velocity of metabolic processes of the p.s.b., no observations had been made until now, as the authors mentioned above in this connection, had restricted themselves to a study of the total metabolism of outgrown cultures.

The results of this first part of my work will be published here. As the experiments will be described in a more extensive way later on, only the most important results will be mentioned and full data will not be given here.

§ 2. *Bacteria and methods used.*

Bacteria. The strains of p.s.b. used, were the numbers *a, b, c, 9* and *19* obtained from F. M. MULLER and strain *d,* isolated by myself from a nearly pure enrichment-culture, handed to me by Dr. H. GAFFRON of Berlin-Dahlem. They were kept as stab-cultures in Burri-tubes and their purity was regularly controled by inoculating peptone-agar plates and incubating these under aerobic and anaerobic conditions in the dark. For the experiments the strains were cultivated in 600 cc. glass-stoppered-bottles with a sterile inorganic medium, containing 0.15 % $Na_2S_2O_3$ and 0.7 % $NaHCO_3$ in the common standard salt solution of VAN NIEL (1931). The pH of this medium was adjusted to 7.8. The bottles were kept in a light-cabinet at a temperature of 30—35° C. All cultures were controled on purity in the way described above, before experiments were started.

Methods. Experiments were made with the aid of WARBURG's common and differential manometers at a temperature of 30° C. ± 0.01°. As light-source provisionally a 50 Watt PHILIPS lamp was used, which allowed two WARBURG-vessels to be illuminated with practically equal intensity.

For each experiment fresh bacteria were centrifuged, washed and suspended in an O_2-free aqueous solution of the necessary inorganic and in a few cases organic salts, which was previously brought in equilibrium with the special gas phase used in each experiment i.e. N_2, H_2 or N_2/H_2-, N_2/CO_2-, H_2/CO_2- or N_2/O_2- mixtures. The pH of this solution was usually adjusted to 7.8, by adding suitable $NaHCO_3$-concentrations. In a few cases CO_2-poising mixtures of 0.1 molar $NaHCO_3$ and Na_2CO_3 were used. It appeared that in all these solutions the bacteria remained in good condition during several hours.

§ 3. *Metabolism of the p.s.b. in the dark.*

When using bacteria, suspended in an aqueous solution of inorganic salts and kept with a N_2 or N_2/CO_2 gas phase in the WARBURG-vessels, there always could be found an increase of volume, quantitatively due to CO_2-liberation, as was shown in a series of experiments an example of which is represented in fig. 1. This is obviously due to a kind of autofermentation. By previously keeping the culture bottles with bacteria in

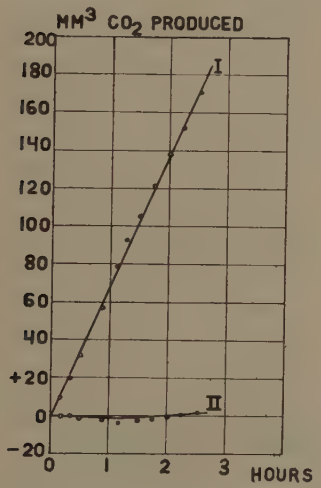

Fig. 1.

I. CO_2-production by bacteria (strain *d*), suspended in 2 % NaCl, 0.3 % $NaHCO_3$ in equilibrium with N_2/7 % CO_2 at a temperature of 30° C.

II. The same arrangement except the presence of KOH in a side bulb of the WARBURG-vessel.

the dark at 35° C. during a day, this autofermentation can be diminished to 30 % of its initial value. It may be stated explicitly that the bacteria before and after this treatment proved to be still very mobile and in good condition.

This CO_2-production was constant during many hours and could not be increased by adding any organic or inorganic substance tried, i.e. peptone, glucose, Na-butyrate, Na-acetate, Na-formiate, Na-malate, Na-succinate, Na-fumarate, Na_2S, $Na_2S_2O_3$, Na_2SO_3, Na_2SO_4.

Consequently no indications were obtained as to the chemical nature of the substance or substances used in this autofermentation process.

Na_2SO_4 is claimed by H. GAFFRON (1934) to be reduced in the dark by p.s.b. to Na_2S, which should be detectable by an increase of CO_2-

production, derived from added organic substances. These experiments were repeated by me in exactly the same way, using the same bacterium [1]) (strain d), cultivated in the same way as used by GAFFRON.

In the fig. 2 and 3, two experiments out of many are represented. From these experiments it is clear that not the slightest influence of the sulfate on the CO_2 production could be detected. Bacteria with reduced auto-

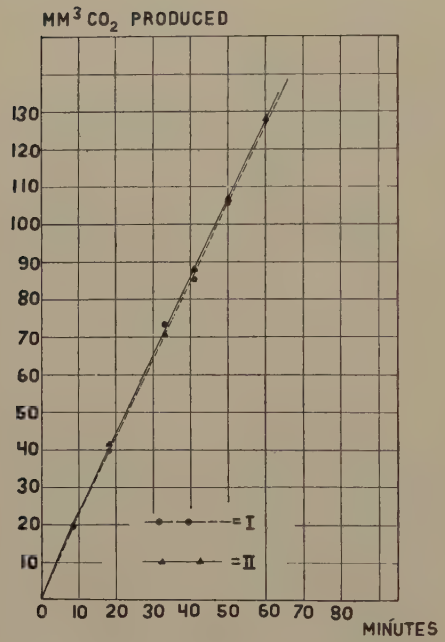

Fig. 2.

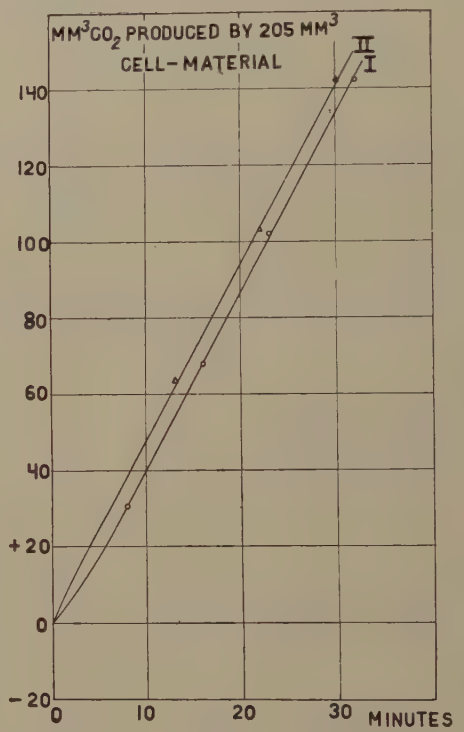

Fig. 3.

Fig. 2.

I. CO_2-production by bacteria (strain *d*), in 2 % NaCl pro analyse, 0.3 % $NaHCO_3$ pro analyse, in equilibrium with N_2/7 % CO_2 at a temp. of 30° C. No SO_4"-ions present.

II. The same arrangement except the presence of 0.2 % Na_2SO_4 and 0.3 % Na-malate.

Fig. 3.

I. CO_2-production by bacteria (strain *d*), suspended in 2 % NaCl pro analyse, 0.3 % $NaHCO_3$ pro analyse, 0.2 % glucose pro analyse, in equilibrium with N_2/7 % CO_2 at a temperature of 30° C. No SO_4" ions present.

II. The same bacteria as in I after addition of 0.5 % Na_2SO_4.

fermentation, being kept in the dark previously, behave in the same way. In this connection I want to lay stress on the fact, that GAFFRON did not

[1]) Perhaps this statement is not quite justified, since I can only say that I used the pure strain *d*, obtained from Dr. GAFFRON's enrichment culture.

use pure cultures and the possibility of contamination with sulfate-reducing bacteria in his experiments has therefore to be considered. GAFFRON's remark that heterotrophic organisms cannot thrive in an inorganic medium, is not justified when autotrophic bacteria are also present.

The experiments described in § 6 of this paper, induced me to study the influence of H_2 on the metabolism of p.s.b. in the dark and I invariably observed, that H_2 is taken up. Compare fig. 6. The velocity of this uptake was constant during many hours and depended chiefly on the quantity of bacteria present and on the concentration of CO_2. Without CO_2 and using bacteria, which after a dark treatment produced only small quantities of CO_2, there was still a slow H_2 uptake, which could be increased many times by using N_2/CO_2 mixtures or CO_2 poising solutions. As CO_2 is always produced, it cannot be considered as strictly proved that no other product of autofermentation can act as a H-acceptor also, but of the organic and inorganic substances tried, i.e. Na-formiate (in conc. of 0.05 %) Na-malate, Na-fumarate, Na_2S, $NaNO_3$ none had any detectable influence. I found however that the p.s.b. are unable to thrive in the dark on a mixture of H_2 and CO_2 as the only assimilation substrate. Still there cannot be any doubt as to a carbon dioxide assimilation in the dark under these conditions.

If oxygen is present in the WARBURG-vessels, it is always taken up with high velocity, which is not surprising when the very low E_h of the medium (see later on) is considered. As p.s.b. are anaerobic, this oxygen-uptake should not be called respiration, as long as it has not been shown that the oxygen consuming process is of physiological significance for the cells.

§ 4. *The carbon dioxide assimilation by p.s.b. in the light.*

a. *Auto-assimilation of CO_2.*

When washed bacteria, suspended in an inorganic medium, devoid of any oxidizable substance are illuminated in suspensions in equilibrium with a N_2/CO_2 atmosphere, they take up CO_2. This goes on rapidly for a short time and thereupon slowly as long as the illumination lasts. After each period of darkness the same phenomenon is observed, as is shown in fig. 4. This can be explained by the assumption that some non-volatile product of autofermentation can serve as hydrogen donator in the carbon dioxide reduction. As a matter of fact this auto-assimilation of CO_2 can be diminished considerably by using bacteria previously subjected to a period of starvation as already mentioned earlier, this treatment having no influence on the assimilatory activity. Otherwise exact experiments on the assimilation of any substance added on purpose, would be impossible, as it seems probable, that the autofermentation goes on in the light also. Perhaps these donators of the auto-assimilation process are identical with the acids shown by GAFFRON (1934, p. 453) to be formed besides CO_2 in the auto-

fermentation process. This author considers the phenomenon described as an assimilation on H_2S, which should be formed out of SO_4'' in the periods of darkness. However in the experiment represented by fig. 4, no SO_4'' was present. Furthermore it may be stated that GAFFRON does not give any proof of the supposed H_2S production in the dark.

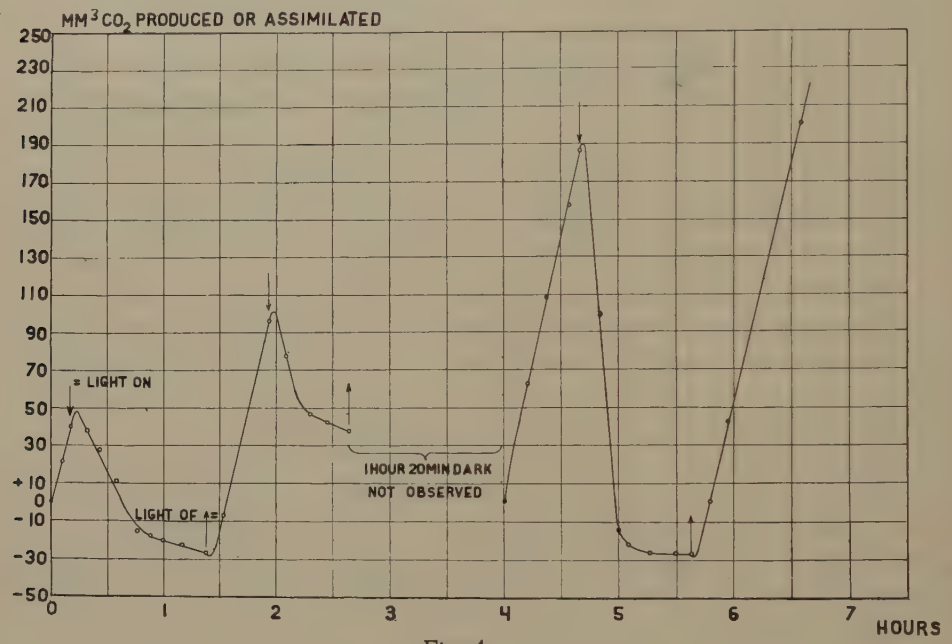

Fig. 4.

CO_2-production and CO_2-assimilation by bacteria (strain *d*), suspended in 2% NaCl pro analyse, 0.3% $NaHCO_3$ pro analyse, free from SO_4''-ions, in equilibrium with N_2/7% CO_2 at a temperature of 30° C.

b. *The CO_2 assimilation with oxidizable S compounds as donators* proceeds rapidly and is constant during many hours. A type experiment is represented by fig. 5. Obviously under the conditions of the experiment two H_2S molecules are used in the reduction of one CO_2 molecule, which means that the oxidation of the H_2S does not go beyond the S stage. This is startling, because the analyses of V. NIEL (1931) have clearly shown that all H_2S can be oxidised by p. s. b. to SO_4'' and as a matter of fact assimilation in WARBURG vessels on $Na_2S_2O_3$ and Na_2SO_3 proceeds rapidly. We have to accept therefore, that the hydratation and following dehydrogenation of the S is a very slow and limiting process in the chain of reactions which together constitute the CO_2 assimilation of the p. s. b.. In agreement with this observation there could not be detected any difference in the auto-assimilation velocity of the same strain of p. s. b. containing or not containing S in their cells.

c. *CO_2 assimilation with organic donators* could not be observed with

any of the substances tried, i.e. glucose, Na-acetate, Na-butyrate and Na-malate, which is in accordance with the observations made by H. GAFFRON

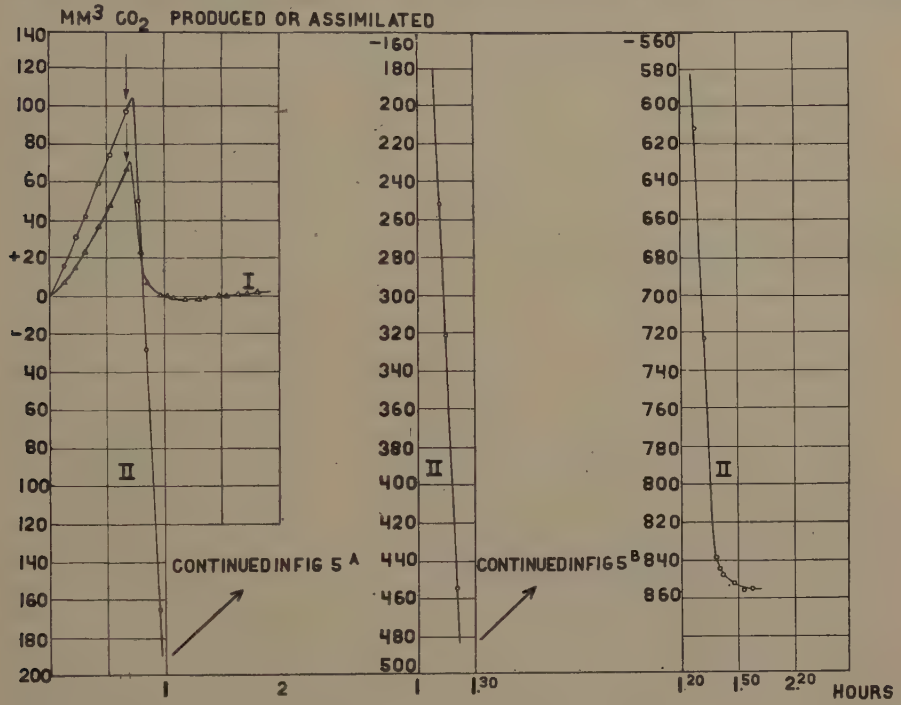

Fig. 5. Fig. 5a. Fig. 5b.

Fig. 5, 5a and 5b.

I. CO_2 production and CO_2-auto-assimilation by bacteria (strain *d*), suspended in 2% NaCl, 0.3% $NaHCO_3$, in equilibrium with N_2/7% CO_2.

II. The same arrangement, except addition of 0.6 cc. saturated aqueous H_2S solution of 14° C. This contains 1650 mm^3 H_2S-gas, which corresponds to 825 mm^3 CO_2 in the case the S is not oxidized further. A rapid assimilation of 870 mm^3 CO_2 is found.

(1934). Yet inoculations from inorganic culture media into organic ones were always successful. So we have to accept that some time consuming adaptation process is indispensable for the organisms under consideration in order to acquire the property of using these organic hydrogen donators. As however, the donators for the auto-assimilation of CO_2 probably also are organic substances, there must be exceptions to this rule. As will be shown later on, Na-malate is such an exception, provided bacteria cultivated in a peptone medium are used.

d. *CO_2 assimilation with H_2 as donator.*

As was already mentioned, H_2 is taken up in the dark, provided CO_2 is present. This process went on much more rapidly when the suspensions were illuminated. which acceleration is probably due to a kind of sensibilisation process. The proportion of H_2 to CO_2 taken up, could not

yet be determined exactly, since large deviations were found in parallel experiments. A type experiment is represented in fig. 6. The decrease in pressure in manometer II is the resultant of the opposing influences of

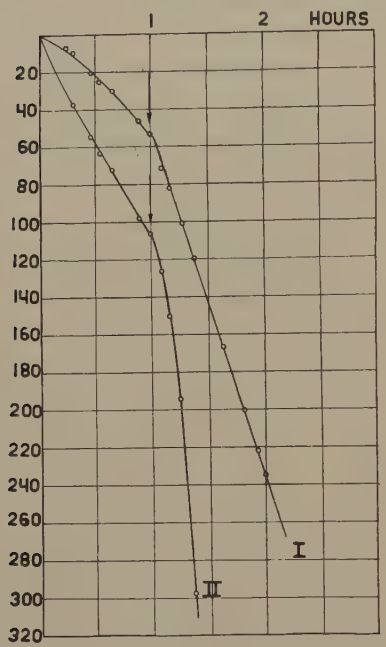

Fig. 6.

I. Change in pressure effected by H_2 uptake in the dark and in the light by bacteria (strain *d*), suspended in 2% NaCl and a CO_2-poising mixture of 9 parts 0.1 molar $NaHCO_3$ to 1 part 0.1 molar Na_2CO_3, at a temp. of 30° C.

II. Change in pressure effected by the gas metabolism of an equal quantity of the same bacteria, exposed to the same light intensity, suspended in 2% NaCl, 0.3% $NaHCO_3$ in equilibrium with N_2/7 % CO_2 at a temp. of 30° C.

The figures at the ordinate represent mM Brodie × 4.4.

CO_2 production (autofermentation), H_2-assimilation and CO_2-assimilation (in the dark only with H_2 as donator, in the light there also will be an auto-assimilation of CO_2).

It was shown in addition that the p. s. b. can thrive on CO_2 and H_2 in the light. A standard salt-solution without an appreciable quantity of oxidizable sulphur compound (Na_2S in a concentration of 0.005 %) kept in an H_2 atmosphere with 8 % CO_2, proved to be very suitable to cultivate p. s. b.

The discovery of this H_2 assimilation means an interesting addition to the series of inorganic and organic donators, suitable for the CO_2 assimilation of p. s. b.

It may be stated incidentally that I also tried a pure culture of the alga *Stichococcus bacillaris* to see whether green plants too are capable of H_2 assimilation. However, the result was negative.

§ 5. *Growth experiments in sodium light.*

Van Niel and Muller (1933, 2) suggested the possibility of an activation of the H-atoms in the donators of the CO_2-assimilation of p. s. b., by means of the red pigment, bacterio-erythrine. Activation of the H-atoms of H_2S, however, would not be necessary, and accordingly green s. b., which only contain a green pigment as far as

known at the moment, can oxidize H_2S to S, but not to SO_4''. Consequently p. s. b., cultivated in light, which is absorbed by their green pigment only, as according to BUDER (1919) for instance the sodium lines at 590 $\mu\mu$, should not be able to oxidize for instance $Na_2S_2O_3$.

This deduction was not confirmed, as culture-bottles, containing 0.2 % $Na_2S_2O_3$ and 0.01—0.02 % Na_2S as donators, grown in light from a sodium lamp, filtered through an aqueous solution of 6 % $CuSO_1$ and 10 % $K_2Cr_2O_7$ (of 1 cm. thickness) did not contain any oxidizable sulphur compound after a few weeks, as shown by iodometric analysis. It seems as though for the CO_2 assimilation, the red pigment can be spared at all, at least for so far as an indirect activation of the red pigment by the green one has not to be taken into consideration.

§ 6. *Experiments with p. s. b. from peptone culture-media.*

Instead of the inorganic medium mentioned above, I also used for some experiments a 1 % peptone medium, pH 8.0. In using these bacteria I obtained the remarkable result that strains cultivated in this way showed marked differences in many respects with bacteria of the same strain cultivated in the normal inorganic medium. The peptone bacteria always show strange big and swollen cells, loose their motility in suspensions in the WARBURG-vessels very soon and have a much lower metabolic activity.

Moreover, they are very sensitive and much more liable to autolysis; they produce H_2 in the dark, could use for instance Na-malate as donator in the light, and effected a lower E_h in the medium both in light as well as in the dark.

a. Metabolism in the dark.

The gas-production per 1 cm^3. cell-material in inorganic suspensions of bacteria cultivated in peptone-broth, always was only about 10—20 % of the gas production of bacteria of the same strain cultivated in the inorganic medium. Furthermore it always appeared, that part of the gas was not CO_2 but H_2, as was shown by absorption of this part by palladium black, put in a side bulb of the WARBURG-vessels. CO and C_2H_2, the only other gases which are adsorbed by Pd in a N_2 atmosphere to any considerable extent, disappear in the presence of a cupro-solution in ammonia, which was not the case with the unknown gas, liberated by the bacteria. The H_2 produced, amounted to 8—30 Vol. % of the total gas-production. The larger the bacteria were and the sooner their motility in the WARBURG-vessels fell off, the more H_2 was produced. So strain 9 as a rule produced 8 % H_2, strain *a* 30 % H_2. As their behaviour towards H_2/N_2 mixtures in the dark, as well as in the light, is the same as in the experiments with bacteria from inorganic culture-media, one has to accept, that part of the cells will produce H_2 while another part will take up this gas. Only differences in velocity of H_2-production and H_2-uptake were

measured in the experiments mentioned. I am inclined to believe that the H_2 production is not a normal physiological phenomenon, but is due to some autolytic process.

b. *Assimilation of CO_2 in the light.*

As for the assimilation of CO_2 with *oxidizable sulphur compounds* and H_2 as donators, there could not be detected any difference with the experiments described above where bacteria from inorganic culture-media were used, except that the assimilation was always much slower in the same light.

The *organic substances* mentioned above in this connection could not be used either, except Na-malate, which apparently could very well act as a H-donator.

These differences are merely indications, that the culture-medium has a marked influence on the properties of the p. s. b. obtained and for this reason one should use bacteria grown in for instance malate or butyrate culture-media, when one wants to study the metabolism in malate and butyrate solutions respectively. Obviously the conclusion of H. GAFFRON (1934, p. 447) about the incapacity of the p. s. b. to use organic substances as hydrogen donators, finds its explanation in the so marked sensibility of the properties of the p. s. b. to the conditions under which they have been grown.

§ 7. *The E_h in suspensions of p. s. b.*

Finally I wish to draw attention to some very preliminary results obtained in experiments regarding the interrelation of the metabolism of the p. s. b. and the oxidation-reduction potential occurring in the medium surrounding the cells.

That such a correlation exists in many cases, has been shown clearly for instance by B. ELEMA (1932), B. ELEMA, A. J. KLUYVER and J. W. VAN DALFSEN (1934), A. J. KLUYVER and J. C. HOOGERHEIDE (1934) and T. Y. KINGMA BOLTJES (1934). Consequently a marked influence of light on the E_h in suspensions of p. s. b. could be expected. At a pH of 8.1, the E_h in the dark in suspensions of p. s. b. from inorganic culture media was as a rule about — 190 millivolt. When light was thrown on the suspension, the E_h increased to about — 100 millivolt within a few minutes. When no hydrogen donator (Na_2SO_3) had been added to the medium, the difference between the E_h in the light and in the dark was much less and soon fell back to some 30 millivolts [1]).

In a way suspensions of the p. s. b. behave therefore as a photo-electric-half cell, provided the substrates for the assimilation are present.

Microbiological Laboratory, Technical University.
Delft, October 1934.

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[1]) The E_h in corresponding suspensions of p. s. b. from peptone broth is ± 100 millivolt lower, both in the light and in the dark, which probably is connected with the H_2-production by these bacteria.

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Botany. — *On a damaging action of substances dissolved from glass in single-cell cultures.* By PETRONELLA S. J. SCHURE. (Communicated by Prof. F. A. F. C. WENT.)

(Communicated at the meeting of October 27, 1934).

In the course of my work on the slime-mould *Reticularia Lycoperdon Bull.* an effort was made to obtain pure cultures of this organism by the germination of single spores in a hanging drop. To this end a moist chamber was prepared by means of a glass ring sealed upon a slide by means of paraffin-oil.

Germination is initiated (in distilled water, at room temperature) after 30 minutes to three hours. The contents swell and the spore-wall is ruptured. The emergent protoplast is at first rounded-off and motionless. After some minutes it begins moving by means of pseudopodia. It becomes elongated and forms a flagellum. This swarm-cell-stage may last for some weeks, in which the swarmers actively move.

The first culture experiments were made in drops containing from one to ten spores in different media. In some drops a large number of spores (some hundreds) was present. Now it appeared that in the drops with few spores (from 1—10), the spores germinated rather well and the swarm-cell stage was attained, but the swarm-cells degenerated within 24 hours. After 48 hours they had disappeared. When some hundred spores were present in a drop the spores germinated regularly and the swarm-cells stayed active for a long time, up to two weeks, depending on the medium.

Experiments in which the number of spores present in a drop differed from one to about thousand evidently show a gradual increase in viability of the swarm-cells. Two sets of experiments were made.

The results of the first set are given in Table 1.

TABLE 1.

Survival of the swarm-cells in drops of distilled water with different number of spores.

Number of drops	Number of spores in a drop	Alive after 24 h.
2	5	0
1	10	0
2	16	1
2	50	1
1	100	0
2	300	1
3	500—1000	3

In drops with 5 and 10 spores the swarm-cells had died within 24 hours, while with 300 and 500—1000 spores in a drop the swarm-cells had survived after the same time. The intermediate experiments with drops containing 16, 50 and 100 spores did not show a relation between the number of the spores and the viability of the swarm-cells.

The results of the second set (Table 2) show a more regular relation between the number of the spores and the viability of the swarm-cells.

TABLE 2.

Survival of the swarm-cells in drops of distilled water with different number of spores.

Number of drops	Number of spores in a drop	Alive after 24 h.
4	1— 10	0
2	25	0
1	100	1
3	300	3
1	500	1
2	500—1000	2

In drops with 1—10 spores and 25 spores the swarm-cells died, while in drops with 100, 300, 500 and more spores the swarm-cells survived after the same period (24 hours).

In the first set of experiments the coverglasses had not always been quite fat-free. In the second set more attention had been paid to the cleaning of the slides.

From these experiments one may conclude that the viability of the

swarm-cells is influenced by the number of the spores in the drop. At the same time an effect of fatty substances on the glass was indicated.

This phenomenon was also observed in the same organism and even to a higher degree, by WILSON and CADMAN. They state that when the spores were present in a small number the germination did not take place or, if it did, the swarm-cells never produced flagella and rounded off almost immediately. Moreover they state that the germination proceeds more satisfactorily when the number of spores is larger. From this fact they conclude that for the germination of the spores and for their development the presence of other swarm-cells is essential. In connection with this phenomenon they mention the theory of the autocatalytic nature of growth (ROBERTSON) in which a hypothetical catalyst should be present and allude to similar phenomena observed in cultures of bacteria and yeasts. No further investigations were made by them to settle the question.

In my opinion it should be investigated whether the presence of living cells themselves is necessary to the swarm-cells or whether some substance, derived from the spores is the causative agent. For this purpose spore-extracts were made in distilled water. They were prepared by heating a dense suspension of spores in distilled water to about 40° C. and filtering this suspension with filterpaper S & S N°. 595 after half an hour.

In these extracts the swarmers stayed alive longer than in the control-drops of distilled water. After 24 hours they always were alive in the drops of spore-extract, while in the blank experiments in distilled water the swarm-cells always had died after that time.

So it appears that the immediate death of the swarm-cells in drops with few spores is prevented by substances derived from the spores. Moreover it appears from the results given in Table 3 that the number of the spores in a drop is of importance to the "span of life" of the swarm-cells.

TABLE 3.

Survival of the swarm-cells in spore-extracts.

Number of drops	Number of spores in a drop	Alive after:			
		1 day	2 days	4 days	6 days
7	1— 10	7	0	—	—
2	< 40	2	0	—	—
2	40	2	2	—	1
4	25—100	4	4	4	—

In the above table we see that in drops containing less than forty spores the swarm-cells had died after two days, while in drops with 40 spores

and from 25—100 the swarm-cells always were alive after two days and in nearly all cases still after a longer time.

Experiments were performed to investigate the action of the substance, extracted from the spores. The following possibilities were taken into account:

1°. The substance is necessary to the swarm-cell to form a part of some structure in the cell, e.g. of the ectoplasmic membrane. Or the substance should possess some regulating function, "autocatalyst" or "bios".

2°. The substance protects the swarm-cell from some toxic influence of the environment, in this case of the glass. The toxic influence emanating from the glass may be physical or chemical. Physical influences which may act on the swarm-cell are the negative electrical charge in the boundary between glass and water, or some interfacial tension in the drop. A chemical influence from the glass may occur by solution of substances from the glass. The possibility of a chemical damage due to dissolved silica was suggested by Prof. H. G. BUNGENBERG DE JONG, to whom I wish to express my hearty thanks for his advice.

The necessity of a specific action of the substance on the swarm-cells was soon eliminated from our considerations, as the "substance" could be replaced by compounds of varying chemical nature. When a layer of paraffin-oil was placed on the coverglasses single spores in drops remained alive. Films of lecithin acted in the same way. To prepare these films a drop of an alcoholic solution of lecithin (MERCK) was placed on the coverglass. After the evaporation of the alcohol a film remained on the glass. On these films single spores in a drop also stayed alive. Later on it appeared that the results with the lecithin films were only positive when old coverglasses had been used. With new coverglasses the swarm-cells died within 24 hours just as on glass. Used coverglasses, even after cleaning with alcali and potassiumbichromate H_2SO_4, acetone and ether, still show traces of fatty substances.

So it was found that the "substance" could be replaced by paraffin-oil and by lecithin together with traces of fatty substances.

Under these circumstances the single swarm-cells, when placed in drops with nutrient solutions remained alive as long as in dense suspensions of swarm-cells. This result was used in some culture experiments. Before cleaning the coverglasses were rubbed with the fingers, and after cleaning with alcali and acid and rinsing with distilled water films of lecithin were placed on it. The action of the fatty substances from the skin was not further investigated.

The following experiment shows that a toxic action of the glass exists which is probably chiefly of a chemical nature. The coverglasses were replaced by other surfaces, differing from the glass in electric charge, surface tension and chemical composition. The results of these experiments are shown in Table 4. The table has been divided in five columns. The first column shows the surfaces replacing the glass. For quartz surface

quartz coverglasses were used. (from Heraeus-Quarzglas. Ges. m. b. H. Hanau a. M.) Also for mica surface mica coverglasses were used. Collodion surfaces were prepared from an ethereal solution of commercial collodion (from N.V. Koninkl. Pharmac. Fabrieken). The starch surface was prepared from a 2 % solution of "amylum solubile" in water. From this solution some wire-loops were placed on glass coverglasses. After evaporation of the water films of starch remained on the coverglasses.

Copper was used as plate-copper.

The paraffin used in this experiment had been purified. An ethereal solution was placed on the coverglasses. The films were dried for half an hour at 100° C. From zein and cetylamin films were prepared respectively from an alcoholic solution and from an aqueous solution. I am much indebted to Mr. G. TH. PHILIPPI who suggested the use of these substances to me and kindly put his preparations at my disposal. Through the kindness of Prof. Ir. P. D. C. KLEY a sample of steel plate V 2*a* was obtained.

The second column shows the sign of the electrical charge between the surface and distilled water. In the third column the degree of wetting by water is indicated; ++ means wetted very well, — the surface is difficult to wet, + is intermediate.

The fourth and fifth column contain, respectively, the numbers of experiments performed and the numbers of experiments in which no toxic action occurred.

TABLE 4.

Survival of the swarm-cells in drops with few spores on different surfaces.

Surfaces used	Electr. charge	Wetting	Number of drops	Untoxic after 24 h.
Quartz	neg.	++	17	3
Mica	neg.	++	6	1
Collodion . . .	neg.	+	20	19
Starch	neg.	++	11	11
Copper. . . .	neg.	+	5	0
Paraffin . . .	indifferent	—	12	9
Zein	pos.	+	16	16
Cetylamin . . .	pos.	+	10	0
Steel V 2a . .		+	3	3

Evidently quartz, mica, copper and cetylamin are toxic. Untoxic are collodion, starch, paraffin, zein and steel V 2*a*. It is evident that the toxicity does not go parallel with the electric charge. Collodion and starch which possess, like glass, a negative electric charge are non-toxic. The

action of the surface tension could be ruled out, for the small depth of the drops on glass and quartz was not damaging in itself. Drops made on paraffin with the same depth were harmless.

So there only remains the supposition of a chemical damage. The existence of such a damage could be proved by placing the spores on a "harmless" surface in drops consisting of distilled water which has been shaken for 36 hours.

Drops of the filtrate, placed on films of paraffin possessed a highly toxic action, comparable to that of glass or quartz.

In this filtrate the presence of silica has been demonstrated according to the method of DIÉNERT and WANDENBULCKE modified by THAYER.

From these investigations we have concluded that in drop-cultures of *Reticularia Lycoperdon Bull.* with few spores in a drop a chemical damage exists due to compounds dissolving from the glass. As the same type of damage exists in drop-cultures on silica, mica and quartz and as, moreover, the substance may be obtained from a solution of powdered glass it seems probable that the damaging substance is silica.

It is possible that not only leucocytes (NIEUWENHUYZEN) and Myxomycetes are sensitive to silica. This would imply a revision of our microscopical method, in which glass covers and slides are universally used.

Leyden. *Botanical Institute.*

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Botany. — *Preliminary studies on the chlorophyll spectrum.* By L. G. M. BAAS BECKING and HENRIETTE C. KONING. (Communicated by Prof. F. A. F. C. WENT.)

(Communicated at the meeting of October 27, 1934).

In the years 1927—1928 we carried out several measurements on the spectrum of chlorophyll, enabled to do so by the kindness of Prof. Dr. L. S. ORNSTEIN, Director of the Physics Laboratory of the University at Utrecht. We want to thank Dr. ORNSTEIN and other members of his staff, particularly Dr. P. H. VAN CITTERT and Dr. M. MINNAERT, for the advice and assistance given to us.

As the result of this work was not entirely satisfactory to us, we decided to postpone its publication until it could be confirmed with more refined methods. Not until last year this program could be realized at Leyden when with the aid of Dr. A. C. S. VAN HEEL and Prof. Dr. H. BUNGENBERG DE JONG, Mr. H. A. BAKKER and Mr. B. HUBERT have started on an analogous programme.

As the latter two investigators intend to publish their results in full, much that will cause repetition shall be left out of this paper, as it is intended as an introduction to the work of the above mentioned investigators.

1. While the chemistry of chlorophyll, owing to the brilliant work of WILLSTÄTTER, STOLL, CONANT and others (1, 4) may be considered to be in a fairly advanced stage, the physical state of this pigment is still very imperfectly known. Several primary spectroscopic facts have not been ascertained, while little has been done on the colloidal properties. Various hypotheses exist concerning the state of chlorophyll in the plastid, which hypotheses seem to be mainly useful in so far as they serve as fixed points, around which experiments may be grouped.

One of these hypotheses, enunciated by K. STERN (3) considers the chlorophyll to be dissolved in "lecithin".

At first this hypothesis seems very plausible, as chlorophyll is fluorescent in solutions as well as in the living plastid.

Our first set of experiments was concerned with the determination of the band position of chlorophyll in various solvents, in order to see whether any solvent could be found which yielded a spectrum similar to that of the living leaf.

The chlorophyll needed for this work was prepared according to the method given by WILLSTÄTTER and STOLL in their "Chlorophyllbuch", using Selaginella and nettle. We have to thank Prof. Dr. F. A. F. C. WENT, at that time Director of the Botanical Laboratory at Utrecht, for his hospitality and the means he placed at our disposal.

We do not flatter ourselves to have obtained pure preparations, as we were ignorant of the rate of photodecomposition of the pigment and as, moreover, the separation from the carotinoids was unsatisfactory. This latter imperfection may have had only secondary importance in relation to the position of the red band.

Solutions of chlorophyll were made, approximately 0.05 %, in various solvents and the spectrum photographed with a small "Fuess" spectrograph, the dispersion-curve of which was determined by means of a Helium-tube. The Helium-spectrum was superimposed several times on every plate exposed, WRATTEN & WAINWRIGHT "Panchromatic" plates were used throughout.

For fluorescence-spectra the light was shot laterally in the cuvette; a ruby glass filter was placed between cuvette and spectroscope.

The fluorescence-spectrum of the living plastid was obtained by filtering the parallel beam of a 1000 Watt incandescent lamp through a layer of 50 cm. 10 % copperchloride. This blue light was reflected from a homogeneous-unicellular layer of the green alga *Hormidium flaccidum*, kindly prepared for us by Dr. T. H. VAN DEN HONERT.

The spectroscope was placed outside the path of direct reflection, the stray light passed through a ruby glass filter before entering the spectroscope slit. One hour's exposure proved to be sufficient in this case.

By means of the dispersion curve the approximate position of the absorption- and fluorescence bands was ascertained. Even with constant slit-width, exposure time and chlorophyll concentration the pictures obtained were extremely variable, the blackening and the width of the band being dependent upon the nature of the solvent in a marked way. Due to this fact and to the imperfect way in which we measured our negatives, the results cannot be regarded as quantitative.

The data show, however, a remarkable tendency, which proved to be sufficiently distinct to allow of certain conclusions to be drawn therefrom.

KUNDT's rule, which states that solvents with high refractive index cause an absorption band to shift to the region of longer wavelength seemed in a general way to be fulfilled.

The second column in Table I shows the position of the band "maximum", while the last column shows the values of n_A of the solvent.

TABLE I.

Name	Abs.	Fluor.	Diel.c.	n_A
Living plant	681	686	—	—
Id. heated	673	681	—	—
Chl. colloidal	677	none	—	—
Dry chl.	695?	none	—	—
Methylene iodide	676	682	—	1.72
Carbon bisulfide	674	681	2.10	1.62
Cedar oil	674	676	1.90	1.53
Benzene	673	675	2.30	1.50
Paraffin	673	678	2.40	1.50
Nujol	672	678	2.00	1.54
Vaselin	672	—	2.40	1.50
Canada balsam	671	—	2.30	1.54
Chloroform	668	680	4.95	1.44
Anilin	667	675	7.32	1.58
Ethyl alcohol	667	675	25.80	1.36
Ethyl ether	666	675	4.35	1.36
Acetone	664	668	21.50	1.38
Lecithin	662	—	—	—
Methyl alcohol	656	666	32.00	1.33

Figure 1 shows the relations. Apart from a few serious discrepancies (anilin, ethyl alcohol) there seems to be a decided shift of the absorption

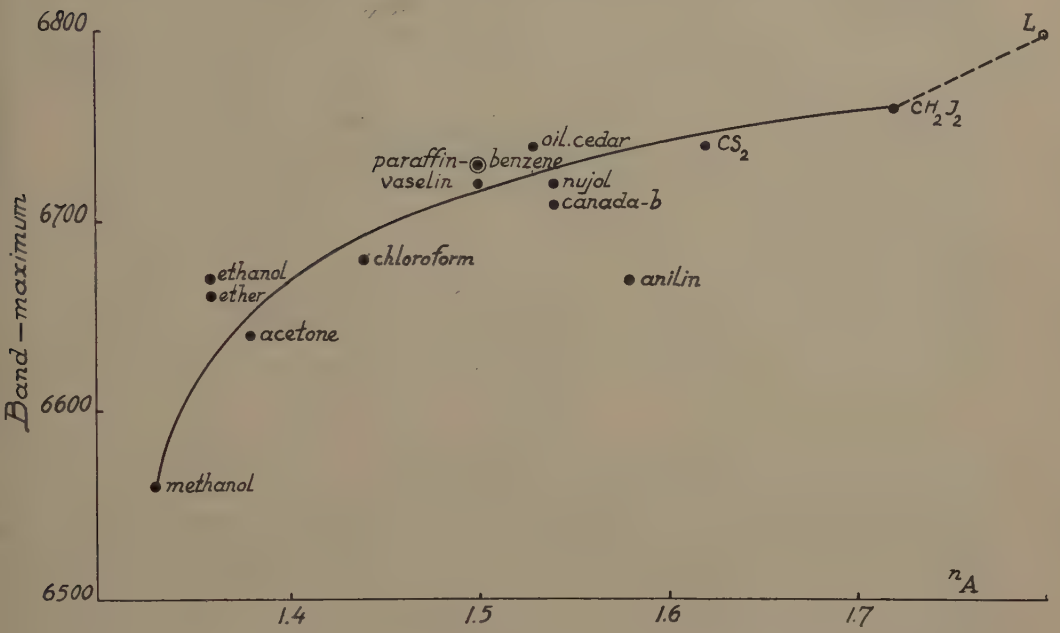

Fig. 1.

band towards the longer wave-length with the increase of the refractive index of the solvent. As mentioned before, no claim is made as to the quantitative validity of these data, still, it may be seen at a glance that no solution can be prepared (unless it would have a "preposterous" refractive index) in which chlorophyll would show an absorption maximum in the neighbourhood of 6800 Å, which would correspond to its position in the living plastid.

Very fine emulsions of dissolved chlorophyll in water, using anilin, benzene and lecithin as a solvent, showed, within the limits of error, the same absorption-maxima as the corresponding unemulsified solutions. As the lecithin solution showed an absorption maximum around 6620 *A*. the "solution-hypothesis" has lost much of its attractiveness.

The maxima of fluorescence show a similar tendency. As measurement of band-position was even more difficult here as with the absorption-spectra, the data only allow us to state that in all cases STOKES' rule seems to be fulfilled, $\nu_1 - \nu_2$ being of the order of magnitude $3-4.10^{12}$.

The spectrum of colloidal chlorophyll showed, within a wide range, independence of *pH*. Only in solutions $[H^+] > 10^{-4}$ changes did occur, probably due to the formation of phaeophytin. In the range *pH* 5—9 the spectrum remained constant.

Heating the plastid (*Hormidium, Diatoms*) caused a marked shift of the red band toward the violet, as observed by WILLSTÄTTER for foliage

leaves. As this heating effect has served MESTRE as a base for his studies on the physical state of chlorophyll in the leaf (2) this point will not be elaborated here.

2. The validity of the BEER—LAMBERT law for solutions of chlorophyll is still a mooted point.

Studies on aqueous colloidal and paraffin-oil solutions of chlorophyll (using P. H. VAN CITTERT's Double Monochromator) showed, that for the wavelengths 675, 725, 785, 870, 1000 and 1165 m μ the relation $I = I_0 e^{-kcd}$ holds true for solutions of chlorophyll in paraffin-oil, while the colloidal solution (which, however, still showed a considerable Tyndall-cone) seemed to follow the law only as far as concentration is concerned. In this case $\frac{I}{I_0}$ seemed to vary inversely with the thickness of the layer.

3. Association of chlorophyll in solutions.

Measurements of the diffusion rate of chlorophyll in solutions of different viscosities should show, for dissolved particles of equal size an inverse proportionality between diffusion rate and viscosity.

The rate is easily observed as the green zone in the liquid is sharply marked. Solutions in methyl alcohol ($\eta_{25} = 0.00738$) and in paraffin-oil ($\eta_{25} = 1.28296$) were allowed to diffuse against gravity in the pure solvents. It appeared that the rate of diffusion of chlorophyll in paraffin oil is so small that it cannot be accounted for by the high viscosity of the solvent. It must be assumed, therefore, that chlorophyll is present in this solvent in much larger particles as in methyl alcohol.

Considerations of the specific extinction in various solvents should take this into account.

Certain other data obtained by us will be used in the subsequent papers. From our preliminary work it appears, however, that it is extremely improbable that chlorophyll should be present in the plastid in the dissolved state. It also appears improbable that it should be present in the colloidal state *per se*. While the position of the absorption band, in this case, seems to be not very different from that in the living leaf, the absence of fluorescence in the colloidal solution seems to point to a fundamental difference. The observations of H. MESTRE (2) seem to point to the existence of a very complex molecule, or micel, of which chlorophyll is only a part.

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Botany. — *Purification of chlorophyll.* By H. A. BAKKER. (From the Botanical Institute, University of Leyden.) (Communicated by Prof. F. A. F. C. WENT.)

(Communicated at the meeting of October 27, 1934).

For the study of the colloid chemistry of chlorophyll and its physical state in the living plant, purification of the chlorophyll, obtained by extraction with acetone from leaves, was needed.

Material.

As investigations started towards the end of the winter only kale (*Brassica oleracea* var. *acephala*; boerenkool) was obtainable in larger quantities, and for that reason used as the source of chlorophyll. Therefore the leaves were dried several days in an air current in drying cells at 40° C., and ground in a ball-mill to a meal of particles of about 100 μ in size. We have to thank Prof. Dr. E. VAN SLOGTEREN and Prof. Dr. G. J. v. ITERSON JR. for the kindness they showed in placing at our disposal the drying chambers and the ball-mill respectively.

Leaves of nettle (*Urtica dioica*), collected at the end of the summer, dried and ground in the same way, showed to contain a good deal of phaeophytin. Fresh leaves of nettle, ground rather coarsely with a food-grinder, were used in some of the later experiments.

Methods.

WILLSTÄTTER and STOLL (**4**, pp. 132—135 and 138—141; also **5**, pp. 247—254) freed chlorophyll *a*—*b* from xanthophylls and other impurities by means of fractionation between two immiscible solvents. The isolation from carotene was obtained by filtering a suspension of chlorophyll in petroleum ether through a layer of talc.

TSWETT (**3**) isolated chlorophyll from the yellow pigments by filtration through sugar, $CaCO_3$ and other adsorbents. The different pigments were adsorbed in different coloured zones and so formed his well-known "chromatogram".

Investigations of KUHN et al. showed the applicability of this chromatographic method to carotenes. The adsorption is determined by the number of oxygen molecules and their bonds.

The adsorption decreases with the decrement of the number of molecules of oxygen.

WINTERSTEIN and STEIN (6) used the adsorption analysis to isolate chlorophyll and to separate the two components.

After several trials the definite method used was a combination of WILLSTÄTTER's and TSWETT's methods. The writer has to thank Prof. Dr. H. J. BUNGENBERG DE JONG, Leyden, for hospitality at his Institute and for much valuable help and criticism.

Solvents.

1. *Acetone.* Distilled over sodium carbonate and sodium sulfite; b.p. 54—58° C.

2. *Petroleum ether.* Distilled with potassium permanganate; b.p. 28—40° C. and 40—57° C.

3. *Ether.* Ph. Ned. V Ad Narcosin was used.

These solvents and methyl alcohol were tested for aldehydes and peroxides after the methods given in "de Nederlandsche Pharmacopee".

Purification method.

1. *Extraction. A. Fresh Leaves.* 250 g of fresh leaves were ground and treated with 100 cc of pure acetone. After 5 minutes the pulp was brought on a Buchner funnel and the acetone removed by suction and pressure. The press cake was again extracted in the same way with 100 cc of acetone. These yellowish green extracts were discarded; they contain much of the yellow pigments but very little chlorophyll, the latter being insoluble in pure solvents (acetone, methyl- and ethylalcohol or petroleum ether), which do not contain *water.*

100 cc of 80 % acetone was added to the pulp and after 5 minutes the deep green extract was filtered by suction and pressure. This treatement was repeated four times with 50 cc of 80 % acetone.

B. Dried leaves. 75 grammes of meal were treated 24 hours with 100 cc of petroleum ether (b.p. 40—57° C.) and the extract, which was obtained in the same fashion as with pure acetone from fresh leaves (and which contains very little chlorophyll and much of the yellow pigments) discarded.

The pulp obtained in either way, was treated with 80 % acetone.

2. *Separation from Xanthophyll.* For this separation WILLSTÄTTER and STOLL's method was used.

The acetone extract was added to 150 cc of petroleum ether (b.p. 40—57° C.) in a glass separatory funnel. To avoid formation of emulsions, which may arise easely and are very annoying, liquids must be added in such a way that they flow slowly down the side of the funnel and during the washing the funnel must be rotated gently. Emulsions formed may be removed to a certain extent with sodium chloride.

50 cc of 80 % acetone were added and the yellowish green acetone-water layer, containing yellow pigments and colourless impurities, was drained

off and discarded. 50 cc of water and 50 cc of 80 % acetone were added and after gentle rotation were drained off; this was repeated once more. 50 cc of water were added and the slight yellowish green acetone-water layer was drained off; this was repeated three times.

Most of the acetone was removed in this way and continued washings with water precipitated the xanthophyll and chlorophyll. In some of the later experiments the chlorophyll was precipitated in this way and separated from xanthophyll by TSWETT's chromatographic method. In other experiments the pigments were left in solution and xanthophyll was removed by WILLSTÄTTER and STOLL's method.

After WILLSTÄTTER's method the chlorophyll was washed five times with 100 cc of 80 % methylalcohol. The xanthophyll and a small amount of chlorophyll *b* were removed in this fashion with the alcohol water layer. It was impossible to remove the xanthophyll quantitatively by this method. WILLSTÄTTER extended the washings with methylalcohol until the alcohol layer was only slightly yellow. After 20 washings, however, this layer still showed a small amount of xanthophyll. This may be proved by saponification of the chlorophyll *b* in the methylalcohol extract (to which has been added some ether) with potassium hydroxide (the phase test), after which the chlorophyll may be removed by washing with water and the methylalcohol remains colourless in the case of absence of yellow pigments.

If methylalcohol of a lower concentration (f.i. 60 %) was used, the xanthophyll precipitated with formation of yellow films.

The petroleum ether solution was washed very cautiously, to avoid emulsions, five times with 100 cc of water to remove the methylalcohol, and to precipitate the chlorophyll, which should be insoluble in pure petroleum ether. The petroleum ether solution loses its fluorescence and is precipitated after washing away the last traces of alcohol, which prevent precipitation. It must be remarked, however, that in our case a total disappearance of fluorescence and complete precipitation could not be observed. Washing with water was repeated more than 10 times and even a continuous washing-method was applied without striking success. We obtained a better result by washing the petroleum ether solution with 100 cc of 80 % acetone before the removal of the methylalcohol, combined with our continuous washing-method. By this method water was squirted from beneath against the petroleum ether-alcohol boundary. The level of the liquids in the funnel was regulated by a siphon.

For the following two reasons: the unsufficient removal of the xanthophyll and the difficulty of a complete precipitation of the chlorophyll by WILLSTÄTTER's method (which difficulty has not, to our surprise, been mentioned in literature), the chromatographic method of TSWETT, which was used by WILLSTÄTTER only to separate carotene from chlorophyll, was applied to remove the xanthophyll. TSWETT's method was also applied to petroleum ether solutions freed from xanthophyll by WILLSTÄTTER's method.

3. *Removal of Carotene and Xanthophyll by* TSWETT's *Chromatographic method.* Cautiously dried cane sugar, mixed with talc (6:1) and rubbed through a sieve, was stirred to a dilute pulp with petroleum ether (b.p. 28—40° C.). This pulp was slid in a glass tube of 3.5 cm diameter and 60 cm length, which was closed at the bottom with a cotton wad and a copper gauze, and was connected with a suction-pump to a suction-Erlenmeyer. This column was exposed for several hours to an effective suction, during which the surface of the absorbent must be covered with petroleum ether to avoid annoying irregularities in the absorption column. (WINTERSTEIN and STEIN l.c. p. 273).

The petroleum ether solution of chlorophyll was sucked through the absorption column. In this way several differently coloured zones appeared in the tube; a chromatogram was developed. If these zones have too great a dispersion, the development of the chromatogram may be stopped by addition of some benzene (freed from thiophene). After washing with 25 cc of petroleum ether (b.p. 28—40° C.) the column was sucked to dryness. Seven zones were found in the chromatogram:

1. A very thin colourless or slightly yellow zone, which contains perhaps some xanthophyll.
2. A thick yellowish to olive green zone, containing most of chlorophyll *b*.
3. A blue-green zone, containing nearly pure chlorophyll *a*.
4. A thin yellow zone, containing xanthophyll.
5. A narrow grayish zone.
6. A narrow yellow zone, containing xanthophyll, and changing imperceptibly into.
7. A more orange-coloured zone, containing the carotene.

TSWETT (l.c. p. 388) ascribed the yellow colour of zone 1 to xanthophyll β, that of zone 4 to xanthophyll α' and α'', which may be violaxanthin after WINTERSTEIN, and that of zone 6 to xanthophyll α, which may be lutein after WINTERSTEIN (l.c. p. 264). Zone 5 was described as colourless by TSWETT and was not observed by WINTERSTEIN.

Starting from a non-fluorescent, almost flocculating petroleum ether solution of chlorophyll, from which the greater part of the xanthophyll was removed with methylalcohol, only zones 2, 3 and a yellow one, containing carotene and a little xanthophyll, were observed in the chromatogram.

WINTERSTEIN compounded mixtures of the pure components, obtained by an elaboration of TSWETT's method, and described the visually observed intensities of the four most important absorption bands of mixtures containing different quantities of these components.

Zone 2 showed to contain a mixture with two nearly equal absorption bands at about 6600 and 6450 Å, that at 6600 Å, however, being the

weakest. This shows that this zone contained about 85 % of component *b* and 15 % of *a*, which conclusion was confirmed by its absorption band at 4450—4600 Å as published in another paper (**1**, Tab. III, No. 28).

Zone 3 contained nearly pure chlorophyll *a*. In a recent paper ZSCHEILE (**7**) ascribes the band observed in chlorophyll *b* by TSWETT as well as by WINTERSTEIN at 6140 Å to a small amount of *a* as an impurity.

The saponification test showed that zone 2 as well as zone 3 contained a little of the yellow pigments. To remove these last traces zones 2 and 3 were extracted with ether and acetone (1 : 1). The acetone was washed away with water, the ether dried by addition of some fused sodium sulfate and evaporated in vacuo. The chlorophyll was dissolved in 5 cc of ether and 100 cc of petroleum ether (b.p. 28—40° C.) and after evaporation of the ether the absorption after TSWETT's method was repeated. The chromatogram consisted only of the zones 2 and 3, preceded by a very slightly yellowish layer. The saponification tests of zone 2 and 3 were negative, so all the yellow pigments were removed.

Component *b* may be separated from *a* by repeating the chromatographic method with the upper half of zone 2 three to five times.

We did not observe in our chromatogram any trace of ZSCHEILE's component *c*, which very probable is a mixture of *a* and *b*. Reasons for this conclusion will be given in a later paper.

SCHERTZ (**2**) found that the phase test of chlorophyll prepared by WILLSTÄTTER's method was positive, the chlorophyll containing a small amount of yellow impurities. He tried to correct this method by repeating the filtration through a thin layer of talc as used by WILLSTÄTTER. We also used this method but the yellow colour did not disappear entirely.

The yields of the combined method of WILLSTÄTTER and TSWETT were very satisfactory. From 75 grammes of dried leaves about 500 mgs of chlorophyll were obtained. The chlorophyll prepared in this way showed, by determination of magnesium, a purity of 97 %. By using SCHERTZ's modification of WILLSTÄTTER's method 405 and 450 mgs were obtained from 250 grammes fresh leaves.

The extraction as well as the purification of chlorophyll were performed as much as possible in the dark, or if light had to be used, a Wratten green darkroom filter was employed.

The chromatographic method used for the separation of components *a* and *b* may have two other advantages as mentioned by WINTERSTEIN: it is much less expensive than WILLSTÄTTER's, which requires large quantities of solvents. Moreover oxidation during the preparation may be avoided.

Summary.

1. The methods of TSWETT and WILLSTÄTTER and some of their

modifications proposed by SCHERTZ and WINTERSTEIN & STEIN were used to extract the chlorophyll and to isolate it from the yellow pigments.

2. The advantages of the chromatographic method of TSWETT combined with the initial phases of WILLSTÄTTER's method were demonstrated.

3. A brief account of the method is given.

4. No trace of a third component of chlorophyll as mentioned by ZSCHEILE was observed.

5. Some yields of chlorophyll are given.

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Botany. — *On the Photodecomposition of Chlorophyll.* By B. HUBERT. (From the Botanical Institute, University of Leyden.) (Communicated by Prof. F. A. F. C. WENT.)

(Communicated at the meeting of October 27, 1934).

As in our experiments the typical "colour curve" of chlorophyll, as given by F. WEIGERT (4) could not be reproduced, neither with the KEUFFEL and ESSER "colour analyzer" nor with a KÖNIG—MARTENS spectrophotometer, it seemed worth while to investigate the reason of this discrepancy.

VAN DEN HONERT (2), using the KEUFFEL and ESSER instrument was unable to duplicate WEIGERT's curve, but ascribes the deviation to contamination of his extracts with carotinoids. The curves given by WURMSER (6) (obtained with the KÖNIG—MARTENS instrument) show a similar deviation.

In contrast with VAN DEN HONERT we believe that WEIGERT's data do not represent the typical "colour curve" of chlorophyll, because of the photolability of his pigment. *It is possible that* WEIGERT *has made use of (at least partly) photodecomposed material.*

In the experiments described in this paper the leaf-extract was prepared as described by WEIGERT; ten grammes of fresh spinach were extracted with 100 *cc* of 85 % acetone and the extinction determined with a KÖNIG—MARTENS spectrophotometer every 15 minutes for the following wavelengths:

7000, 5900,
6800, 5500,
6440, 5000,
and 6250, 4800 Å.

The results are shown in Figure 1.

Here $\log kcd = \log E$ of BEER—LAMBERT's law * $\left(*\log \frac{I_0}{I} = kcd\right)$ is plotted against wavelength ("colour curve"). The shape of this curve

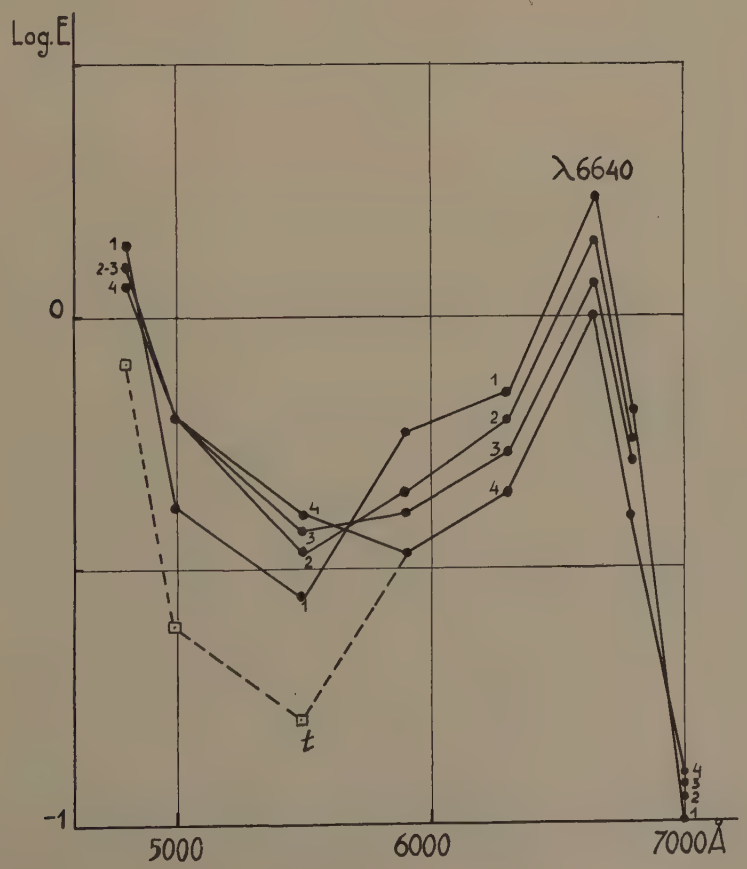

Fig. 1.

is independent of concentration as, by reduction of the concentration to $\frac{1}{n}$ of its value $\log E = \log kcd - \log n$, and every point of the curve is therefore deplaced along the ordinate over a distance equal to $\log n$. The truth of this statement depends, of course, upon the applicability of BEER—LAMBERT's law to solutions of chlorophyll. From our own experience this law seems to hold in first approximation. Small deviation, however, seems to have been detected (WURMSER), while colloidal solutions showing a Tyndall cone do not follow the law (1). Photodecomposition should go parallel with a change of $\log E$, and if d is kept constant, the only remaining variable is c. Plotting $\log E$ against observation-time (Figure 2) shows that:

$\log E$ decreases between λ 5900 and λ 7000 while between λ 5000 and λ 5900 $\log E$ seems to increase. This decrease in $\log E$ should be due to a disappearance of the chlorophyll while the increase in $\log E$ at the shorter wavelengths could be brought about by the appearance of the photo-decomposed product.

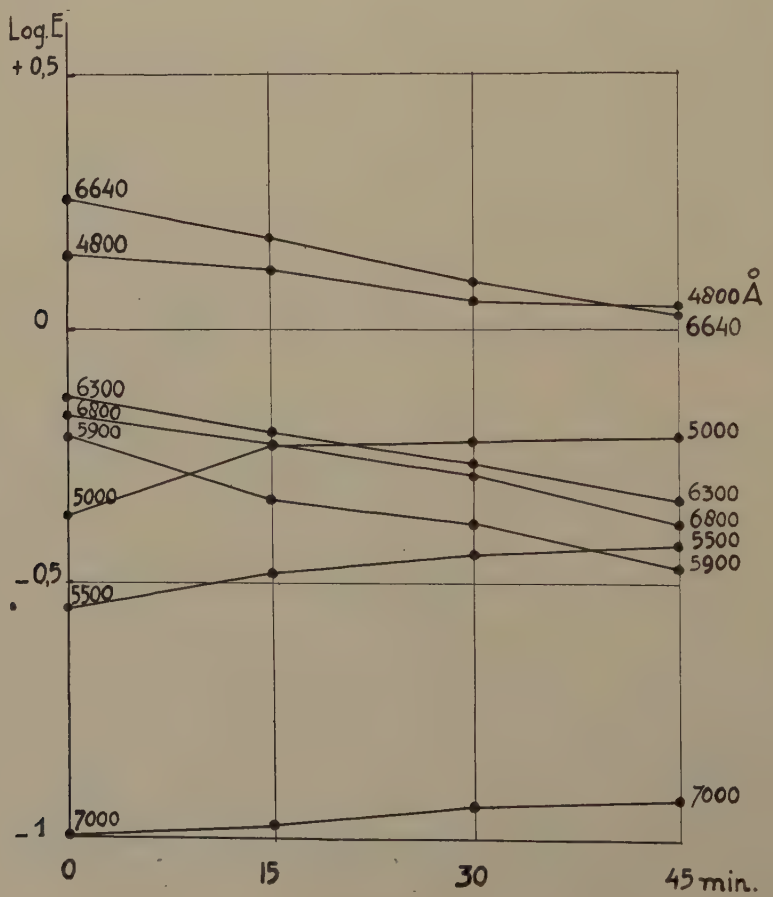

Fig. 2.

We may observe, moreover, that in the range of 6250—6800 Å the decrease in $log E$ is homogeneous and, therefore, strongly suggests a diminution in concentration, while, in the range of shorter wavelengths, no such law seems to hold. At 7000 Å an *increase* in concentration of some substance seems to be indicated.

Now if we follow curve 4 Figure 1 from the red end towards the blue, we might extrapolate (along the dotted line) a theoretical curve, which curve represents a reduction of chlorophyll corresponding to that found for λ 6640. Obviously the ordinate distance point for point between this dotted curve and the *observed* curve 4 represents a spectrum characteristic of the photodecomposed pigment.

When we call the extinction E and the corresponding (extrapolated)

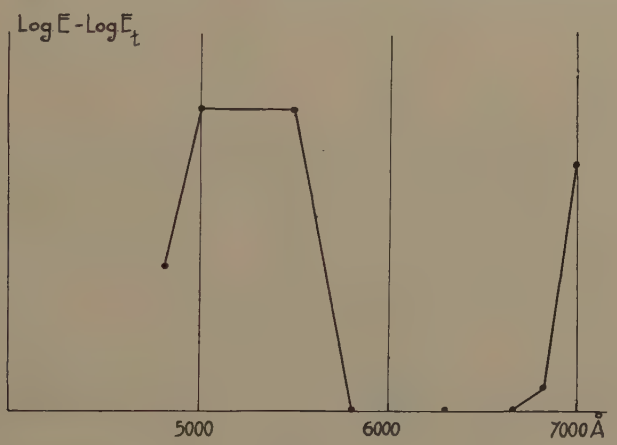

Fig. 3.

extinction E_t, the relation between λ and $(log E - log E_t)$ should indicate, in a rough way, the "colour-curve" of the photodecomposed substance. $\lambda = f\ (log E - log E_t)$ is represented graphically in Figure 3.

If the photo-oxidation-product absorbed radiation in the region 6000—7000 Å, the chromatographic analysis of TSWETT (3) might show more than two coloured zones.

Using the modification of this method as given by WINTERSTEIN (5) and ZSCHEILE (7), three distinct zones could be observed in a glucose-talcum column when working in full sunlight; two green zones appeared but also a grey zone. This grey zone, already described by TSWETT might correspond to the photodecomposed chlorophyll.

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Botany. — *Notes on the colloid chemistry of chlorophyll.* By H. A. BAKKER. (Communicated by Prof. F. A. F. C. WENT.)

(Communicated at the meeting of October 27, 1934).

Introduction.

It is a well known fact that the absorption bands of a solution of chlorophyll as compared with those of the pigment in the living plastid shift to the shorter wave length of the spectrum.

This may be explained by the solution of chlorophyll in a medium of a very high index of refraction in the plastid. According to investigations of BAAS BECKING (1) and HUBERT (3) it is improbable, however, that so high a value of this change may be attained in this way.

HERLITZKA (2) and WILLSTÄTTER & STOLL (8) observed the absorption bands of colloidal chlorophyll in the same position as in the living plant.

IWANOWSKI, who was the first to assume the colloidal state of the chlorophyll in the plastid (4 and 5), found as the results of quantitative measurements of the bandposition of chlorophyll with a spectrophotometer, the bands of colloidal chlorophyll to be intermediate between that of a true solution and that in the plastid. By means of addition of electrolytes to his suspensoid he obtained the same absorption band in the red as observed in the plastid (5). As WILLSTÄTTER added some magnesium carbonate to his solutions, to avoid the splitting- off of magnesium by hydrolysis in dilute solutions, and as his observations were not quantitative, IWANOWSKI's observations may be the most trustworthy.

In contrast to a true solution of chlorophyll and the pigment in the plastid, a suspensoid of chlorophyll shows no fluorescence. NOACK (7) supposed therefore the chlorophyll to be adsorbed on some protein in a

monomolecular layer in the plastid. He obtained fluorescent adsorbates of chlorophyll with WILLSTÄTTER's "aluminium hydroxyde C" and with globin. B. BECKING & H. C. KONING (oral communication), however, could not obtain fluorescence in casein and talc-adsorbates.

It must be mentioned too that a suspensoid of chlorophyll does not yield the chlorophyll to ether, benzene or carbon disulfide but after addition of very little electrolyte. This agrees with the insolubility of the pigment from the plastid in pure organic solvents, which do not certain any water.

According to these facts it may be supposed that the chlorophyll in the plastid is in a colloidal state, which is not, however, a simple one, such as an emulsion or suspension.

Experimental.

1. *Charge and stability of suspensoids of chlorophyll.* Cataphoresis of a suspensoid of chlorophyll, obtained by squirting an acetone extract of leaves of fresh nettle (*Urtica dioica*) in destilled water and removing the acetone by dialysis, showed it to be negatively charged. Flocculation was observed with hexol [1]), luteo [2]), magnesium chloride and sodium chloride. Being exposed to the light for several hours the sol showed a positive charge, which may be explained by photooxidation.

A suspensoid of phaeophytin, prepared in the same way, also showed a negative charge.

The suspensoids of both chlorophyll and phaeophytin were discharged with hexol and charged positively by addition of more of this electrolyte.

The Reciprocal Hexol Number, that is to say the number of grammes of colloidal substance which will be brought to the zero point by 1 gramme-equivalent of hexol ions, was calculated to be 860 for chlorophyll and 320—400 for phaeophytin.

The following experiments were made with suspensoids of chlorophyll and phaeophytin from which the acetone was removed by evaporation in vacuo. The chlorophyll was purified by WILLSTÄTTER's method. The flocculation by several electrolytes was observed by light absorption with MOLL's Extinctionmeter. Some of the results are represented in Table I, and in figures 1 and 2.

The values of the absorption in parenthesis have been caused by the absorption of luteo itself.

From this it may be clear that the sols of chlorophyll and phaeophytin are hydrophobic. The flocculation is markedly dependent upon the valence

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1) Hexol nitrate $= [Co\{Co(OH)_2en_2\}_3](NO_3)_6$.

2) Luteocobalt chloride $= Co(NH_3)_6Cl_3$.

TABLE I.

Influence of electrolytes on a sol of 0.0025 % of chlorophyll and a sol of 0.00 5% of phaeophytin.

Light absorption in %								
Sol	Chlorophyll 0.0025 %				Phaeophytin 0.005 %			
Neutral salt	KCl	$CaCl_2$	Luteo	Hexol	KCl	$CaCl_2$	Luteo	Hexol
Final concentration of neutral salt in mill. aeq.								
1600	25				66			
800	28	28			67	64		
400	29	31			68	68		
200	30	31.5			56	68		
80	20	31.5			48	37		
40	12.5	32			38	30		
20	12	32	30	(31)	28	24	69	(69)
8	11	12	33	(26)	23	36	69	(62)
4	11	12	34	(18.5)	20	29	63	(52)
2	11	12	34	(13.5)	20	24	40	(43)
0.8			32	(12.5)			31	34
0.4			26	12.5			26	33
0.2			14	12			24	31
0.08			13	12.5			22	38
0.04			12	15			20	67
0.0192				16				61
0.0160				16.5				
0.0128				17				
0.0096				17				
0.0080				17.5				
0.0064				16				
0.0040				12				
0.0032				12				
0.0016				12				
Blanco	11	11	11	11	10	10	10	10

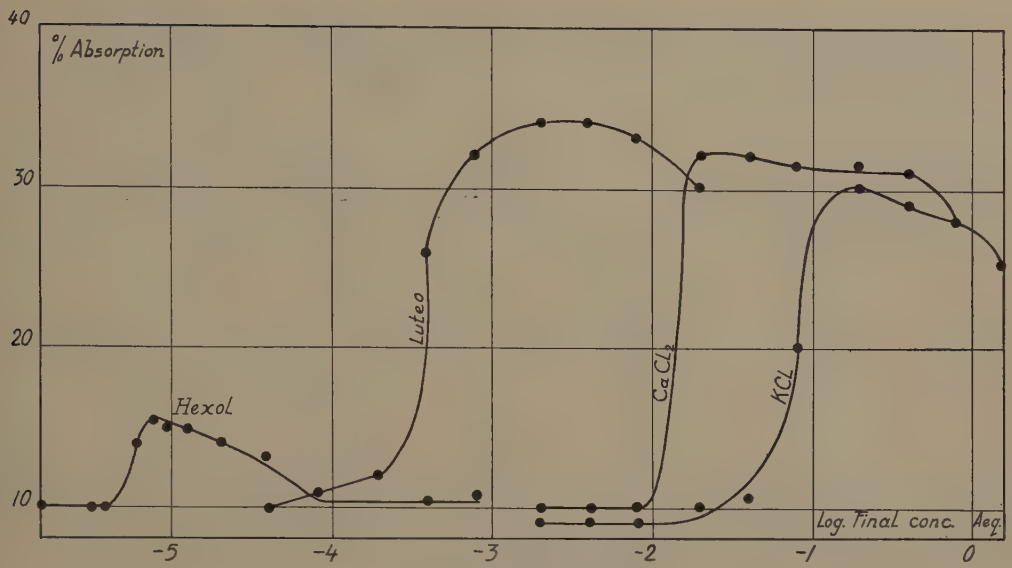

Fig. 1. Chlorophyll

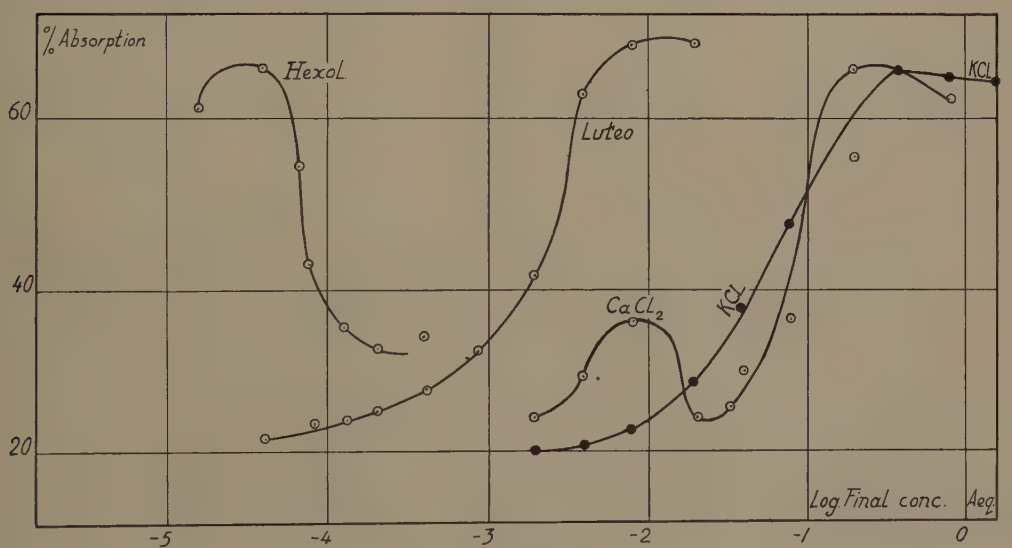

Fig. 2. Phaeophytin

of the cations, according to the SCHULZE—HARDY rule. This may be demonstrated by the results represented in Table II and in figure 3.

There is an irregularity in the course of the curve of $CaCl_2$ of the sol of phaeophytin, which occurs also in a curve of a sol of chlorophyll which contained a small amount of phaeophytin.

The maximum of absorption of hexol has an important lower value than that of the other salts, which may be ascribed to a chemical reaction of the complex-ion with chlorophyll, for it was not observed in phaeophytin.

TABLE II.

Influence of bivalent cations on a suspensoid of 0.00212 % of chlorophyll.

	Light absorption in %.			
Neutral bivalent salt	$BaCl_2$	$CaCl_2$	$SrCl_2$	$MgCl_2$
Final concentration of salt in mill. aeq.				
800	33—	33	31	34
400	34.5	35	28+	31—
200	35	36	30+	32
80	35	37+	36+	37+
40	35	37+	36	38—
20	36—	35.5	35+	20
8	14.5	13.5	14—	14—
4	13	13—	13	13.5
2	13—	12.5	13	13.5
0.8	12+	12+	12+	13
0.4	12	12.5	12+	13
0.2	12	12	12+	13
Blanco	12	12	12+	13

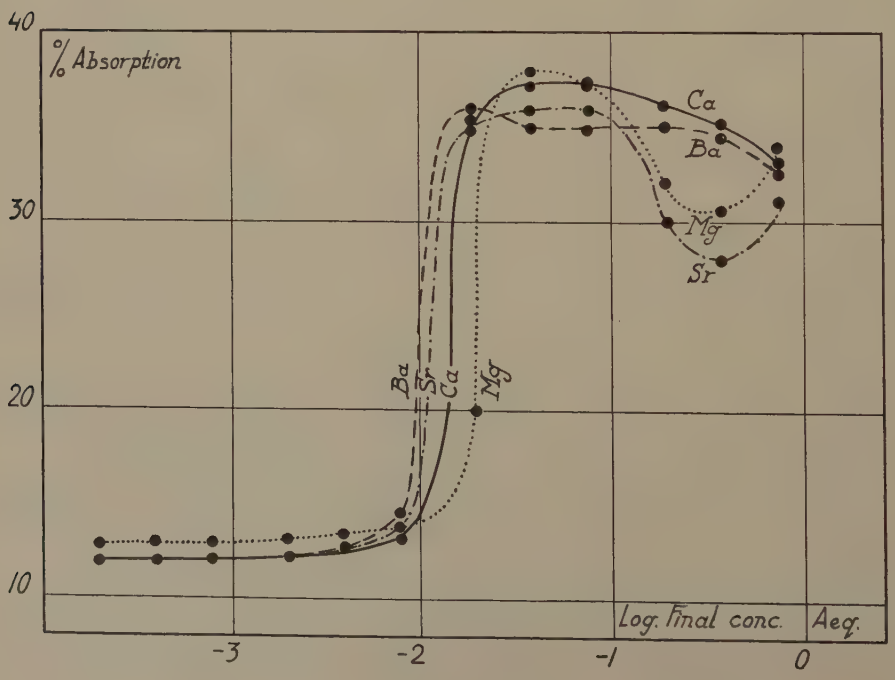

Fig. 3.

The Reciprocal Hexol Number, calculated from the absorption maxima of 3 differently concentrated suspensoids of chlorophyll, amounted to about 2000. As the maximal capacity may not coincide with a condition of zero charge, this number, however, may be considerably in error.

2. *Fluorescent sol of lecithin chlorophyll.* 300 mg of lecithin were dissolved in 11 cc of alcohol. 1 volume of this solution was mixed with 1 volume of an alcoholic solution of chlorophyll.

To 1 volume of this mixture was added water of 80° C, and a sol was obtained which showed a distinct fluorescence.

The results will be found in Table III.

Chlorophyll solution added to lecithin sol showed a small trace of

TABLE III.

Fluorescence of different lecithins with chlorophyll and with different solvents.

Source of Lecithin	Solvent	Fluorescence
Egg	Isopropyl-alc.	+
Soy-bean	„	+
„ „	Ethylalcohol	+
Planticin (RIEDEL & DE HAËN)	„	+
Egg	Pyridin	Opalescence

fluorescence, while neither chlorophyll sol added to lecithin sol, or chlorophyll sol added to a lecithin solution showed any fluorescence.

3. *Fluorescent coacervate of chlorophyll with lecithin.* 1 gramme of soy-bean lecithin was dissolved in 30 cc of isopropylalcohol 66 %, and to 1 volume of this solution 1 volume of a solution of chlorophyll was added. 4 volumes of water of 80° C were mixed with 1 volume of the lecithin-chlorophyll solution and a fluorescent sol was obtained. This sol was precipitated with 0.1 N NaCl, and after being centrifuged chloral hydrate 30 — 40 % in 0.1 N NaCl was added. Slightly green, homogeneous globules arose, which showed a distinct fluorescence in dark-field illumination, and desintegrated in a direct electric current. It may be very probable that in this way an autocomplex coacervate of lecithin with chlorophyll was formed.

Chlorophyll proved to be insoluble in 30 — 40 % chloral hydrate.

This investigation was carried out in the laboratory of Medical Chemistry, Leyden. I want to express my feelings of gratitude tot Prof. Dr. H. BUNGENBERG DE JONG and Mr. W. DEKKER for much valuable help and criticism.

Summary and Conclusion.

1. The sols of chlorophyll and phaeophytin are negatively charged, hydrophobic suspensoids, in agreement with the facts obtained by HERLITZKA.

2. The reciprocal hexol numbers of chlorophyll and phaeophytin as obtained from their cataphoretic velocity are 860 and 320—400. With MOLL's extinction meter a reciprocal hexol number of about 2000 may be calculated for chlorophyll.

3. A fluorescent sol and coacervate of lecithin with chlorophyll was obtained.

4. As lecithinoids may be important components of the plastid (6), the occurence of chlorophyll in a coacervate of lecithin in the living plant should be considered as a serious possibility.

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Botany. — *Estimation of the band position of chlorophyll in different media.* By B. HUBERT. (From the Botanical Institute, University of Leyden.) (Communicated by Prof. F. A. F. C. WENT.)

(Communicated at the meeting of October 27, 1934).

Many authors have tried to give an explanation of the shifting of the band maxima of chlorophyll, and several theories have tried to account for the facts observed. The principal theories are:

1. Chlorophyll is in true solution in the plastid.
2. It is dispersed in small particles in the stroma.
3. It is in the colloidal state.
4. It is adsorbed on some protein.

The first three theories are purely based on spectroscopical or spectrophotometrical evidence.

A careful examination of the values found for the maxima gives a very confused idea.

The following table (p. 696), which is far from complete, demonstrates this fact very clearly.

From the many possible errors we may mention a few.

1. URSPRUNG (17) gives a simple explanation of the low value found by DONATH (4).

If a wide slit is used (one jaw moveable) a shifting of the band may easily be obtained.

Working with a HÜFNER-spectrophotometer he found the maximum in alcohol at λ 6610, widening the slit displaced the maximum to λ 6370.

2. Readings must be taken 20 Å apart, at critical points 5 Å apart.

3. When a monochromator is used the width of the second slit may give rise to serious errors.

SEYBOLD worked with a slit permitting 100 Å to pass (15).

His maxima are shifted to the blue end of the spectrum (acetone λ 6500).

Our method consisted in photographing a continuous spectrum with and without chlorophyll.

The spectrograph used was designed by Dr. A. C. S. v. HEEL (8) and shows the desirable characteristics of this instrument. Moreover, instead of one, two prisms, filled with ethyl-cinnamate, were used.

The author is much indebted to Dr. VAN HEEL for his great help and advice, to the Firm of Kipp & Sons for their kind assistance, but especially to Mr. A. J. STUIVENBERG, master-mechanic at the Botanical Laboratory, Leyden, and his assistant, Mr. H. VIJLBRIEF, who spend more than seven months in the construction of this instrument.

As the instrument will be fully described at another place, a few remarks must suffice here.

Despite the presence of two prisms, the dispersion is not very great, as is shown by comparison of the dispersion curves of our spectrograph and the small "Fuess" instrument used by BAAS BECKING. (Fig. 1.)

Before the slit was placed a "Stufenabschwächer" constructed of three pieces of smoked glass permitting to photograph the spectrum simultaneously at 100, 65, 39 and 26 % intensity.

Smoked glass acts as a grey filter, it is only slightly selective in the range used by us.

The lamp used was an OSRAM-NITRA 15-watt lamp, running on a 6 volt storage battery (100 amp-hours).

A voltmeter was inclosed in the circuit to control the light-intensity.

Before the lamp was placed a red filter to take away the false light.

A line spectrum having no lines at the place were we expected the band, was thrown over the continuous spectrum. (He or Na). On the

Author	Medium	Bandmaximum	Instrument
D. IWANOWSKI	Living leaf	λ 692—682	Spectrophotom.
A. URSPRUNG	„ „	„ 675	Spectrogr.
D. IWANOWSKI	„ „	„ 690—680	Spectrophotom.
REINKE	„ „	„ 676—663	„
v. GULIK	„ „	„ 673—666	„
ENGELMANN	„ „	„ 665—660	„
DONATH	„ „	„ 638	„
BAAS-BECKING	Euglena	„ 670	Leitz Microsp. Oc.
B. BECKING and	Aspidistra	„ 6810	Fuess Spectrogr.
J. KONING	Impatiens	„ 6810	„
„ „	Hookeria	„ 6810	„
„ „	Selaginella	„ 6810	„
„ „	Hymenophyllum	„ 6810	„
A. SEYBOLD	Phaseolus	„ 6700	Fuess Monochrom.
B. HUBERT	Aquilegia	„ 6810	Spectrogr. v. Heel
„ „	Sapium	„ 6810	„ „ „
„ „	Desmodium	„ 6810	„ „ „
„ „	Wistaria	„ 6795	„ „ „
„ „	Iatropha	„ 6800	„ „ „
IWANOWSKI	Alcohol	λ 6760—6620	Spectrogr.
B. BECKING	„	„ 6675	Fuess Spectrogr.
HUBERT	„	„ 6680	Spectrogr. v. Heel
F. WEIGERT	Acetone	λ 6640	
WÜRMSER	„	„ 6700	Spectroph. König—Martens.
B. BECKING	„	„ 6640	Fuess Spectrogr.
BAKKER and HUBERT	„	„ 6640	Keuffel a. Esser Col.-anal.
„	„	„ 6660	Spectroph. König—Martens.
„	„	„ 6665	Spectrogr. v. Heel.
SEYBOLD	„	„ 6500	Fuess Monochrom.

same plate we put a NEON-spectrum for the construction of the dispersion curve.

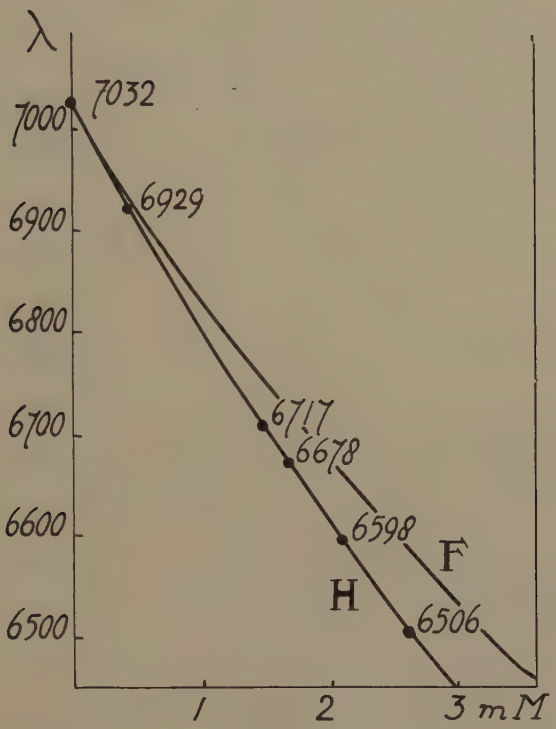

Fig. 1

Table of lines used.

Neon	Helium	Sodium
7032.4	7065	5895.9
6929	6678	5889.9
6717		
6678		
6598		
6532		
6506		

As the chief advantage of the instrument is its great luminosity, exposure-times of 30—180 seconds (slid-width 0.19 mm., light-intensity 50 Lux on the slit) sufficed. In this short period of time no convection-currents occurred in the prism-liquids.

The negatives were measured by means of a MOLL microphotometer, kindly put at our disposal by Prof. Dr. H. DORGELO, Director of the Laboratory of Technical Physics at the Technical Institute, Delft.

The following table gives the results of our experiments.

Solvent	Bandmax.	N_D^t of solvent
Methylalc.	6625	1.329
Ether	6640	1.352
Acetone	6665	1.358
Ethylalc.	6680	1.361
Benzol	6720	1.508
Living leaf		
Wistaria multijuga	6995?	
Iatropha multifida	6800	
Aquilegia glauca	6810	
Desmodium girans	6810	
Sapium spec.	6810	

In graph. 2 the band-positions are plotted against the refractive indices of the solvent.

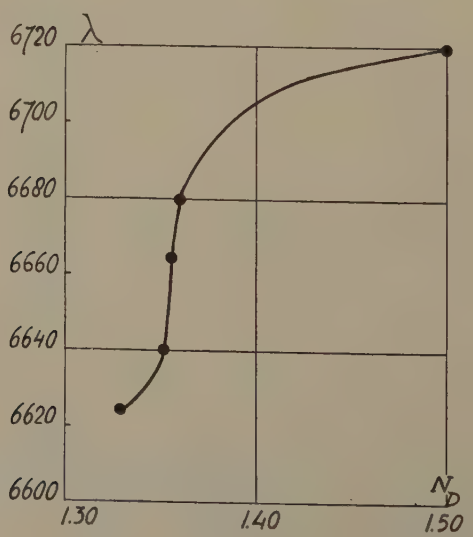

Fig. 2

The following conclusions are allowed:

1. The shifting apparently follows KUNDT's law.

2. If chlorophyll is dissolved in the plastid, it must be in a medium of very high refractive index.

3. There are many points in favour of the adsorption theory put forward in different forms by WILLSTÄTTER (19), (20), NOACK (12), MESTRE (10), MULLER (11) and others, which we hope to discuss in another paper.

Aug. 1934.

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Anatomy. — *Margoide Entwicklung des tuberculum dentis.* Von TH. E. DE JONGE-COHEN. (Communicated by Prof. M. W. WOERDEMAN).

(Communicated at the meeting of October 27, 1934.)

Selbständige Entwicklung eines *tuberculum dentis* bei den vorderen Zähnen — in gewissem Sinne das morphologische homologon des *tuberculum anomale* der Molaren — ist ein bekanntes und bereits von vielen älteren Untersuchern beschriebenes Phänomen [MÜHLREITER (I), ZUCKERKANDL (II) u.a.].

In seinem normalen Bau häufig nichts anderes als eine mit dem blossen Auge kaum wahrnehmbare Randleiste, kann es sich — namentlich beim lateralen oberen incisivus, aber auch nicht selten beim cuspidatus — in extremen Fällen zu einem Element mit solch einer scharf ausgeprägten Eigenart entwickeln, dass dadurch in deutlicher Weise die dimere Structur der Frontzähne zu Tage tritt. Morphologisch konnten wir diese Form als *„Prämolarisation"* der Schneidezahnkrone bezeichnen (III).

Viel weniger bekannt ist es, dass daneben erst von BOLK (IV) ein zweiter Typus beschrieben worden ist, bei dem eine Leiste die Verbindung aufrechterhält zwischen Deuteromer (*tuberculum dentis*) und Protomer (*margo incisalis*): dies scheint übrigens, von einigen gelegentlichen Mitteilungen abgesehen — wir nennen an erster Stelle COLYER (V) und GOTTARDI (VI) — der einzige Hinweis zu sein, den wir darüber in der Literatur finden. Grund genug, die Beschreibung von BOLK, die zudem den Ausgangspunkt zu unseren eigenen Betrachtungen bildet, in extenso wiederzugeben.

„Bekanntlich" — so BOLK [1]) — „kann beim Menschen das Deuteromer — besonders des lateralen Incisivus — sich bisweilen ausserordentlich entwickeln. Es gibt dabei zwei Modificationen: entweder bleibt das stark entwickelte Deuteromer (tuberculum dentis) mit der lingualen Fläche vom Protomer verbunden, oder es ragt als freier selbständiger Höcker hervor. Im ersteren Falle erlangt die Schneide des Zahnes *eine eigentümliche T-förmige Gestalt* [2]) und an der Wurzel des Zahnes ist von einer Furchung wenig oder nichts zu sehen."

Wie gesagt, das Wesentliche der Abweichung sehen wir darin, dass, wenn das Deuteromer sich selbständig entwickelt, sich zugleich eine Leiste bildet, die eine ununterbrochene Verbindung herstellt zwischen vestibulärer Fläche und tuberculum dentis. Sehr auffallend dabei aber ist eine — offenbar consecutive — von basal nach incisal allmählich zunehmende Krümmung der vestibulären Kronenfläche gerade dort, wo diese Fläche in

[1]) loc. cit. pag. 46.

[2]) Wir cursivieren.

Verbindungsleiste bzw. tuberculum übergeht: dass diese Krümmung ausgeprägter erscheint, je nachdem auch die Verbindungslinie stärker entwickelt ist, ist gewiss mehr als eine zufällige Coincidenz.

Das markanteste Beispiel dazu gibt uns Tafelfigur 2: bei beiden Schneidezähnen findet sich eine so scharfe Krümmung, dass es aussieht, als wäre die vestibuläre Fläche in zwei Hälften geteilt; zugleich beobachten wir eine solche Prominenz der Verbindungsleiste bzw. des tuberculum, dass letzteres sich einfach zur margo incisalis specialisiert hat: es ist dieser Entwicklungsmodus, der ausgedrückt werden soll durch die Umschreibung *„margoide Entwicklung"*.

Geht aus Obigem hervor, wie einschneidend die Entwicklung der Verbindungsleiste die ganze Configuration der Schneidezahnkrone zu ändern vermag, noch complicierter wird das Bild, sobald das Deuteromer sich in zwei Höcker spaltet. Beispiele dazu geben uns die Abb. 4, 5 und 7 und bei aufmerksamer Betrachtung scheint hier nicht nur die vordere Fläche — wiederum in derselben Höhe, wo die crista sich ansetzt — gekrümmt, sondern auch die aus den beiden Höckern aufgebaute orale Fläche bietet einen ähnlichen Anblick: der Dreiweg Y ist der Form einer Honigwabe ⋎ gewichen.

Dass beide Typen teratologisch zu den seltensten Deformitäten gehören, die bis jetzt bei den prämaxillären Gebisselementen gefunden worden sind, braucht wohl kaum hervorgehoben zu werden — um so merkwürdiger ist darum auch der gewissermassen universelle Character der Anomalie, welche fast alle theoretischen Entwicklungspotenzen zu verwirklichen scheint.

Zeigt uns doch unser Material, dass sie sich in ihrer Entwicklung nicht nur auf den Oberkiefer beschränkt, sondern auch im Unterkiefer vorkommt. Ausserdem tritt sie nicht nur im permanenten Gebiss, sondern gleichfalls bei den Milchzähnen auf. Endlich aber — und dies ist wohl sehr bemerkenswert — manifestiert sie sich in beiden Dentitionen noch bei überzähligen Gebisselementen (Abb. 6 und 7).

Dass sie in unserem vorletzten Fall (Abb. 6) den rudimentären Character trägt, der nun einmal dem mesiodens immer eigen ist, liegt eigentlich wohl ganz auf der Hand.

Dass wir in der Gelegenheit waren, im Museummaterial des anatomischen Laboratoriums nicht weniger als neun Fälle ausfindig zu machen, bietet uns den willkommnen Anlass, wieder einmal Herrn Professor Dr. WOERDEMAN unsren besonderen Dank abzustatten für die liebenswürdige Art und Weise, wie er uns immer bei unsren Untersuchungen unterstützt.

ZUSAMMENFASSUNG.

Beschrieben und abgebildet wurde eine Reihe von Fällen, bei denen die Entwicklung einer Leiste eine ununterbrochene Verbindung herstellte zwischen Protomer (vestibuläre Kronenfläche) und Deuteromer (tuber-

culum dentis). Characteristisch war dabei eine Krümmung der vestibulären Fläche in der gleichen Höhe, wo sich die crista ansetzt. Dadurch, dass sich diese Dysmorphie sowohl im Ober- und Unterkiefer, im bleibenden wie im Milchgebiss, an normalen wie an überzähligen Gebisselementen manifestierte, bewies sie deutlich ihren universellen Charakter.

SUMMARY.

A number of instances were described and illustrated, in which the formation of a crest brought about an uninterrupted connection between the protomer (vestibular crown surface) and the deuteromer (tuberculum dentis). A feature in this case was a crack in the vestibular surface precisely at the juncture of the crest.

Whereas the manifestation in upper and lower jaw, with milkteeth and permanent rows of teeth, in the case of normal and of superfluous elements, throws a clear light upon the universal character of the dysmorphy, described above.

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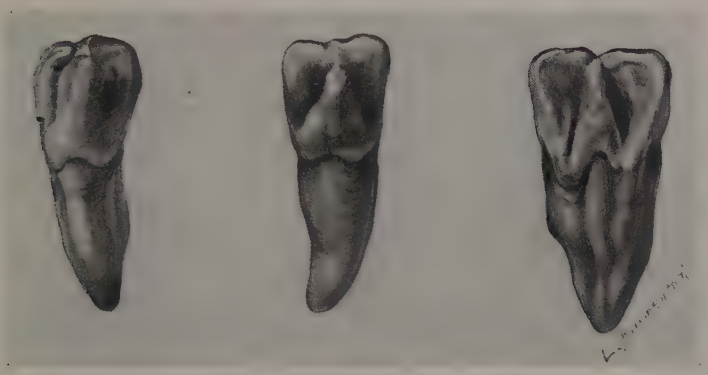
ERKLÄRUNG DER TAFEL.

I. *Stufenweise Entwicklung des margoiden tuberculum dentis.*
Fig. 1. *a* und *b*: laterale Schneidezähne im Oberkiefer; *c*: medialer Schneidezahn im Oberkiefer.

II. *Margoide Entwicklung nur eines tuberculum.*
Fig. 2. Mediale Schneidezähne im Oberkiefer (bilateralsymmetrischer casus).
Fig. 3. Medialer Schneidezahn im Unterkiefer.

III. *Margoide Entwicklung zweier tubercula.*
Fig. 4. Lateraler Schneidezahn im Unterkiefer.
Fig. 5. Lateraler Schneidezahn im Oberkiefer der Milchdentition.
Fig. 6. Überzähliger Zahn — mesiodens — im permanenten Gebiss.
Fig. 7. Überzähliger Zahn im lactealen Gebiss.



a *b* *c*

Fig. 1

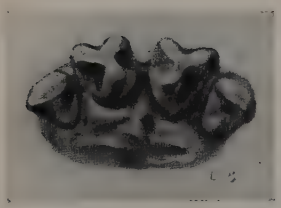


Fig. 2

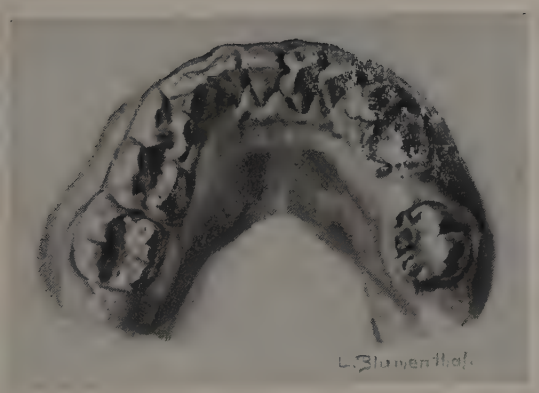


Fig. 3

Präparat aus dem Museum-GREVERS.

Fig. 4

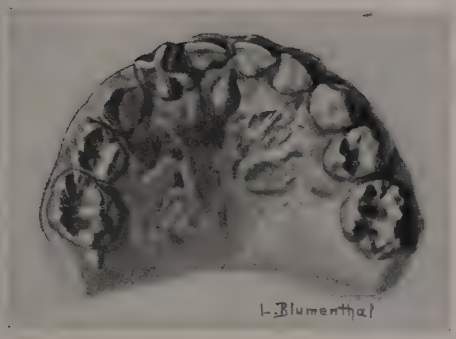


Fig. 5

Fig. 6

Fig. 7

Anatomy. — *Ueber das Vorkommen kreuzender Fasern im Facialis und Glossopharyngeus.* Von TOSHIYUKI KUROTSU (Kaiserl. Univ. Osaka). Aus dem Niederländischen Zentralinstitut für Hirnforschung) (Communicated by Prof. C. U. ARIËNS KAPPERS).

(Communicated at the meeting of October 27, 1934).

Vor kurzem hat ADDENS (1934) in der Festschrift für KAPPERS eine kritische Uebersicht gegeben über das Vorkommen kreuzender Fasern im Facialis, Vestibularis, Glossopharyngeus und Vagus. Er gelangte dabei zu den folgenden Ergebnissen.

Weder in der sensiblen noch in der cerebrospinalen motorischen VII-Wurzel gibt es kreuzende Fasern. Die öfters bei den Säugetieren beschriebenen kreuzenden VII-Fasern, welche aber auch als zum Vestibularis gehörig oder auf noch andere Weise gedeutet wurden, sind Speicheldrüsenfasern, wie zum ersten Male von KOHNSTAMM (1902) nachgewiesen.

Auch bei den Vögeln gibt es solche kreuzenden Fasern.

Vom Vestibularis weist nur der vestibulo-cerebellare Anteil eine partielle Kreuzung auf.

Keine effektorischen IX- und X-Fasern kreuzen. Dies scheint auch zuzutreffen auf die Speicheldrüsenfasern zur Parotis.

Es gibt keine kreuzenden Fasern in den sensiblen IX- und X-Wurzeln auszer in der Commissura infima.

Diese Folgerungen von ADDENS fuszten grösztenteils auf ein ausführliches Studium der Literatur. Persönlich untersuchte er in Hauptsache nur eine CAJAL-Serie eines siebentägigen Hühnchenembryos.

Es ist nun meine Aufgabe gewesen an Hand der Seriensammlung des Instituts die Verbreitung der VII-Kreuzung durch die ganze Vertebratenreihe zu ermitteln. Während dieser Untersuchung gelang es mir bei den Säugern auch kreuzende Fasern in der IX-Wurzel nachzuweisen, die m. E. zur selben Kategorie wie die kreuzenden VII-Fasern gehören.

Für die folgenden Ausführungen habe ich, weil am bequemsten für die Darstellung, die umgekehrte phylogenetische Reihenfolge gewählt, werde also mit den Säugern anfangen.

Säugetiere.

Der Facialis. Dasz es im Bereiche des VII-Knies kreuzende Fasern gibt, steht auszer Frage, wiederholt sind dieselben im normalen Präparat, sowohl beim Embryo wei beim erwachsenen Tiere, beschrieben und auch durch die MARCHI-Methode dargestellt worden. In Fig. 1 bilde ich sie

für den Hund ab. Sie treten mit dem Vestibularis ein, verlaufen in lockeren Bündeln dorsomedialwärts und etwas nach vorne, durchqueren die moto-

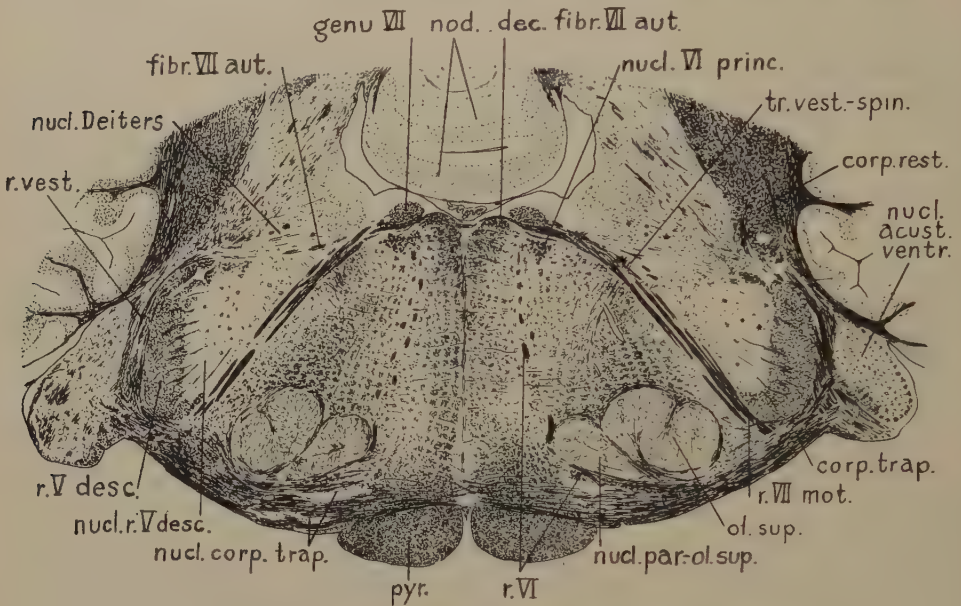


Fig. 1. *Hund.* Querschnitt durch die Kreuzung der autonomen VII-Fasern. WEIGERT—PAL-Parakarmin. Auf der linken Seite sieht man die gekreuzten Fasern sich dem Vestibularis zugesellen.

rische VII-Wurzel, ziehen unter dessen Knie hindurch und treten schliesslich als eine kompakte Faserschicht zwischen medialem Längsbündel und Ventrikel in die andere Seite über.

Während also die Anwesenheit dieser Fasern keinem Zweifel unterliegt, ist ihre Deutung sehr umstritten. Wie schon eingangs erwähnt, sind sie nicht allein als VII-, sondern auch, weil mit diesem Nerven eintretend, als Vestibularisfasern oder auf noch andere Weise gedeutet worden.

Es ist nicht meine Absicht hier eine vollständige Uebersicht aller Deutungen dieser kreuzenden Fasern zu geben. Ich verweise auf die Abhandlungen von RASMUSSEN (1932, p. 154—155) und ADDENS (1934). Letzterer hat u. a. auseinandergesetzt, dasz CAJAL (1909 und vorhergehende Schriften) sich in eigentümlicher Weise geirrt hat, indem er nebeneinander das Bestehen kreuzender VII- und Vestibularisfasern annahm. Auch WINDLE (1933 und vorhergehende Arbeiten) ist hierzu geneigt, während SHANER (1934) es tatsächlich tut.

Persönlich möchte ich mich der Meinung derjenigen Autoren anschliessen, die in den betreffenden Fasern Speicheldrüsenfasern erblicken, mit dem Vorbehalt aber, dasz neben solchen Fasern auch autonome Fasern anderer Art an dieser Kreuzung teilnehmen können, ja dasz die Kreuzung sogar ganz aus anderen autonomen Fasern bestehen kann. Weiter unten werde ich dies aus vergleichend-anatomischen Gründen zu erweisen ver-

suchen. Ich werde mich daher im Folgenden der umfassenderen Bezeichnung autonome Fasern bedienen.

Dasz die in Frage stehenden Fasern Speicheldrüsenfasern sind, wurde zum ersten Male behauptet von KOHNSTAMM (1902, '02*a*). Nach Durchschneiden der Chorda tympani bei Hunden fand er Chromatolyse in dorsal vom motorischen VII-Kern liegenden Zellen, und zwar zumeist auf der kontralateralen Seite. Diese Befunde konnten nur teilweise von YAGITA und HAYAMA (1909) bestätigt werden, welche keine Kreuzung fanden und auch eine etwas verschiedene Lage der verletzten Zellen angeben.

Vor kurzem hat aber KAIDA (1929) beim Kaninchen nicht allein nachgewiesen, dasz die mit dem Vestibularis eintretenden Fasern teilweise unter dem Ventrikel kreuzen, aber auch, dasz sie nach dem Ganglion geniculi gehen und von da in die Chorda tympani. Nach KAIDA ist nur ein Drittel dieser Fasern gekreuzt, während KOHNSTAMM annahm, dasz die Mehrzahl kreuzte. Mittels Durchschneidens der Chorda tympani erzielte KAIDA Chromatolyse in der dorsalen retikularen Formation, auch zum gewissen Grade auf der heterolateralen Seite.

LORENTE DE NÓ (1922) gelang es in GOLGI-Präparaten von Mausembryonen die in Frage kommenden präganglionären Fasern zu imprägnieren. Er fand, dasz einige davon gekreuzt waren.

In der Hauptsache können m. E. die Behauptungen KOHNSTAMM's, die lange keine genügende Beachtung gefunden haben, hierdurch als bewiesen gelten.

Was nun die Frage der Verbreitung des kreuzenden Bündels anbetrifft, aus der von RASMUSSEN (1932, p. 154—155) gegebenen Uebersicht geht hervor, dasz es bisher von verschiedenen Autoren bei den meisten gewöhnlichen Laboratoriumssäugern angetroffen ist (Hund, Kaninchen, Katze, Maus). Weiter wurde es wiederholt für den Menschen erwähnt. VORIS und HOERR (1932) beschrieben es für das virginische Opossum und SHANER (1934) für das Schwein. KAPLAN (1913) untersuchte in seiner Arbeit über die spinale Acusticuswurzel 61 Säuger aus verschiedenen Ordnungen, gibt aber nicht im einzelnen an, wo er das kreuzende Bündel (sein Fasciculus vestibularis medialis) antraf. Er sagt nur, dasz es bei den Carnivoren besonders gut ausgebildet ist.

Persönlich konnte ich die Kreuzung der autonomen VII-Fasern bei sämtlichen daraufhin von mir untersuchten Säugern feststellen. Fast alle dazu benutzten Serien waren gefärbt nach WEIGERT—PAL, zumeist mit Nachfärbung von Parakarmin.

Die untersuchten Arten sind folgende:

Monotremen: Echidna aculeata, Ornithorhynchus anatinus; Beuteltiere: Didelphys marsupialis, Hypsiprymnus murinus, Macropus giganteus melanops, M. robustus, Phascolomys latifrons; Insektenfresser: Erinaceus europaeus, Talpa europaea; Chiropteren: Vesperugo noctula, Xantharpyia amplexicaudata; Xenarthra: Choloepus didactylus, Myrmecophaga jubata, Tamandua tetradactyla;

Nagetiere: Cynomys socialis, Kaninchen, Marmota spec., Sciurus vulgaris; Raubtiere: Crossarchus obscurus, Hund, Katze; Pinnipedier: Phoca vitulina; Bartenwale: Balaenoptera sulfurea; Zahnwale: Phocaena communis, Tursiops tursio; Elefanten: Elephas indicus; Sirenen: Manatus spec.; Paarhufer: Schwein, Tragulus meminna; Unpaarhufer: Tapirus indicus; Halbaffen: Lemur catta; Platyrrhine Affen: Ateles hybridus, Cebus fatuellus, Leontocebus rosalia, Oedipomidas oedipus; Catarrhine Affen: Nemestrinus nemestrinus, Anthropopithecus troglodytes; Mensch.

Auf Grund obiger Liste, die Vertreter fast sämtlicher Säugerordnungen umfaszt, darf man wohl schlieszen, dasz die VII-Kreuzung ohne Ausnahme durch die ganze Klasse vorhanden sein wird.

Oben habe ich schon darauf hingewiesen, dasz m. E. die Kreuzung nicht ausschlieszlich aus Speicheldrüsenfasern zusammengesetzt ist, ja dasz sie sogar ganz aus autonomen Fasern anderer Art bestehen kann. Das letztere musz bei den Cetaceen der Fall sein.

Diesen Tieren fehlen Speicheldrüsen gänzlich. Trotzdem traf ich bei allen drei untersuchten Vertretern der Ordnung (Balaenoptera sulfurea, Phocaena communis, Tursiops tursio) die VII-Kreuzung an. Hier musz es sich also handeln um Fasern anderer Art, vielleicht um solche zu den Tränendrüsen und Schleimdrüsen der Nase und Mundschleimhaut, die auch im VII-Nerven verlaufen. Es liegt auf der Hand anzunehmen, dasz auch bei den übrigen Säugern derartige Fasern neben den Speicheldrüsenfasern anwesend sind.

Meistens ist die Kreuzung der autonomen VII-Fasern auf das VII-Knie beschränkt, kann sich aber auch weiter caudalwärts ausdehnen. Beim Seehund und Tapir findet die Kreuzung sich auch zwischen den beiden horizontalen Schenkeln der motorischen VII-Wurzel vor. Bei Cebus, Nemestrinus, Oedipomidas und Ornithorhynchus kann man die kreuzenden Fasern sogar zwischen den aufsteigenden Schenkeln der VII-Wurzel finden, so dasz sie eine ununterbrochene Schicht mit den ihnen nach hinten folgenden kreuzenden IX-Fasern bilden.

Oedipomidas zeigt ganz interessante Verhältnisse in dieser Hinsicht. Bei ihm sind die kreuzenden VII-Fasern in zwei Gruppen, eine craniale und eine caudale, geteilt. Jene überschreitet die Medianebene, wie im allgemeinen, in der Kniegegend, diese aber in der Gegend des Crus ascendens.

Bei Balaenoptera und Tursiops fand ich zwar die Kreuzung vor, konnte aber keine Fasern zum Vestibularis verfolgen. Hier möchte ich annehmen, dasz die kreuzenden autonomen Fasern die Medulla zusammen mit der motorischen VII-Wurzel verlassen.

Der Glossopharyngeus. Die Anwesenheit kreuzender Fasern in den IX- und X-Wurzeln bei Säugern, wie auch bei Vögeln, ist besonders von CAJAL behauptet worden, der sich dabei im Gegensatz zu VAN GEHUCHTEN befand.

Anfänglich dachte CAJAL, dasz die betreffenden Fasern motorisch waren und aus den Nucleus ambiguus hervorgingen, aber später änderte er seine Ansicht und betrachtete sie als sensibel. ADDENS (1934) gelangt zum Schlusz, dasz vom IX- gar keine und vom X-Nerven allein sensible Fasern in der Commissura infima kreuzen. Nur über die Speicheldrüsenfasern zur Parotis, die mit der IX-Wurzel austreten und deren Kern nach YAGITA (1909) und LORENTE DE Nó (1922) die direkte caudale Fortsetzung des Speicheldrüsenkerns der VII-Wurzel ist, hat ADDENS Bedenken gehegt wegen des Betragens der Fasern aus letzterem Kern, so dasz er des Fehlens kreuzender IX-Fasern doch nicht ganz sicher ist. Auf Grund der Arbeiten WINDLES (1933) über die Entwicklung des Katzenhirns, der hier keine kreuzende IX-Fasern angab, und auch weil er selbst beim Hühnchenembryo keine derartigen Fasern fand, hält ADDENS es aber am Ende doch für wahrscheinlicher, dasz kreuzende Fasern zur Parotis fehlen. Auszerdem hatte YAGITA (1909) nach Durchschneiden des Ramus tympanicus bei Hunden nur Degeneration im Kerne der operierten Seite gesehen.

Wie schon eingangs erwähnt ist es mir gelungen in der IX-Wurzel das Vorhandensein kreuzender Fasern nachzuweisen, die m. E. zur selben Kategorie wie die kreuzenden VII-Fasern gehören (Fig. 2, 3).

Erst nachträglich ersah ich aus der Literatur, dasz schon LUNA [1]) (1930) kreuzende IX- und X-Fasern bei Vesperugo kuhli und JELGERSMA (1934) kreuzende IX-Fasern bei einigen Wassersäugetieren beschrieben und abgebildet hatten.

Nach ihrem Eintritt verlaufen die kreuzenden IX-Fasern zusammen mit den anderen motorischen Fasern (Fig. 2) oder auch mit den motorischen und sensiblen Fasern beide (Fig. 3) dorsomedialwärts und meistens etwas nach vorne. Sie berühren dabei den ventralen Rand des Nucleus fasciculi solitarii und treten am oralen Pole des XII-Kernes vorbei oder durch diesen in die anderen Seite über. Die IX-Kreuzung ist nicht so mächtig wie die VII-Kreuzung und weist weniger Variationen auf.

Bei den höheren Säugern treten die kreuzenden Fasern vereinigt mit dem Rest der motorischen Fasern ventral von der absteigenden V-Wurzel aus oder durchbohren diese (Fig. 2), bei den Monotremen und Beuteltieren dagegen (Fig. 3) verlassen sie die Medulla zusammen mit der gemischten Wurzel dorsal von der absteigenden V-Wurzel. Das letztere ist der ventralen Verschiebung der V-Wurzel zuzuschreiben.

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[1]) Die Abhandlung LUNAs kam erst zu meiner Kenntnis, als die Untersuchung schon abgeschlossen und das Manuskript grösztenteils fertiggestellt war. LUNA gibt bei Vesperugo kuhli nach Silberpräparaten an, dasz aus dem vorderen und mittleren Drittel des Nucleus ambiguus spärliche, aus dem hinteren Drittel aber die Mehrzahl der Fasern in die andere Seite übertritt. So sollte es hier nicht allein kreuzende IX-, sondern auch kreuzende X-Fasern geben. In einer WEIGERT—PAL-Serie von Vesperugo noctula und in einer mit HEIDENHAINs Hämatoxylin gefärbten Serie von Xantharpyia amplexicaudata habe ich dies noch nachprüfen können und glaube auch die kreuzenden X-Fasern gesehen zu haben. Jedenfalls ist aber die IX-Kreuzung deutlicher. Die Zeit fehlte das ganze Serienmaterial auf diesen Punkt zu untersuchen.

Wo die kreuzenden IX-Fasern ihren Ursprung haben konnte ich nicht feststellen, nehme aber gemäsz meiner Deutung an, dasz sie aus dem

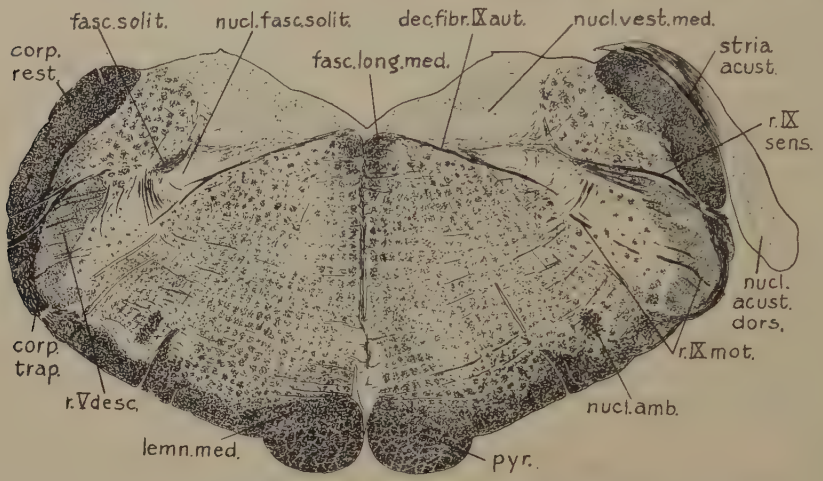

Fig. 2. *Hund.* Querschnitt durch die Kreuzung der autonomen IX-Fasern. Etwas oral vom XII-Kern. WEIGERT—PAL-Parakarmin.

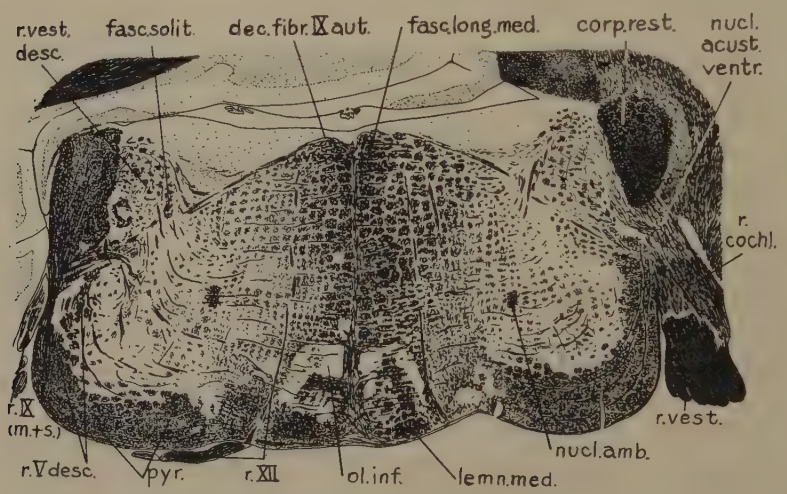

Fig. 3. *Echidna aculeata.* Querschnitt durch die Kreuzung der autonomen IX-Fasern. WEIGERT—PAL-Parakarmin.

Hinterende des Nucleus salivatorius kommen. Wie schon erwähnt dachte CAJAL anfänglich, dasz sie aus dem Nucleus ambiguus hervorgehen (vgl. Fig. 311, p. 749 im I. Bande seines Handbuchs). Später aber ist er an ihrer Deutung irre geworden und hielt sie für sensibel.

Ob CAJAL wirklich die kreuzenden motorischen IX-Fasern gesehen hat, ist fraglich (vgl. die Figuren 6 und 7, S. 276 seiner Arbeit 1909*a*). ADDENS (1934) hat die Vermutung ausgesprochen, dasz die von CAJAL beschriebenen und abgebildeten Fasern vielleicht den sekundären IX- und

X-Bahnen zum Kleinhirn zugehören, die mit der MARCHI-Methode beim Meerschweinchen von ALLEN (1923) und beim Kaninchen von KRAUSE (1930) dargestellt wurden und welche teilweise gekreuzt sind. Mir ist es am wahrscheinlichsten, dasz CAJAL beide Arten von Fasern gesehen hat. Ohne Silberpräparate, die mir nicht zu Gebote standen, ist dies aber nicht zu entscheiden.

Die Serien, die ich für die Untersuchung der IX-Wurzel benutzte, waren dieselben wie im Falle der VII-Wurzel (vgl. die Liste S. 4 u. 5). Auszerdem stand mir eine WEIGERT—PAL-Parakarmin-Serie vom hinteren Ende der Oblongata von Halicore dugong zur Verfügung.

Bei der weitaus überwiegenden Zahl der untersuchten Arten konnte ich die Kreuzung nachweisen, obgleich öfters nicht ohne Schwierigkeit, nur bei Choloepus [1]), Manatus, Sciurus, Talpa und Mensch war mir dies nicht möglich. Auch beim Seehund fand ich die kreuzenden Fasern nicht, doch war die untersuchte Serie nach VAN GIESON gefärbt. JELGERSMA (1934) gibt sie aber für dieses Tier an, wie auch für Lutra vulgaris und Phocaena communis. LUNA (1930) hat sie bei Vesperugo kuhli gesehen.

Aus obiger Aufzählung geht jedenfalls hervor, dasz die IX-Kreuzung weit verbreitet ist unter den Säugern, sogar neige ich der Annahme zu, dasz sie wie die VII-Kreuzung hier ohne Ausnahme vorhanden ist. Das Vermissen in den wenigen Fällen möchte ich der ungenügenden Färbung der benutzten Serien oder dem Aufgelockertsein des kreuzenden Bündels zuschreiben Ist es doch schwer zu glauben, dasz die Kreuzung allen untersuchten Affen zukommt und beim Menschen fehlen sollte.

Wie oben angegeben liegt die IX-Kreuzung meistens vor der Austrittsebene der Wurzel. Bei den Cetaceen liegt sie aber in dieser, so dasz man hier in einem Schnitt fast den ganzen Verlauf der kreuzenden Fasern beobachten kann.

Im allgemeinen besteht eine Lücke zwischen den kreuzenden VII- und IX-Fasern. Falls aber die ersteren sich weit nach hinten ausdehnen wie bei Cebus u. s. w. (vgl. S. 4), oder umgekehrt falls die IX-Kreuzung wie bei Nemestrinus weit nach vorne bis in die Gegend des Crus ascendens des VII-Nerven verschoben ist, dann ist eine scharfe Abgrenzung der beiden kreuzenden Wurzeln unmöglich oder schwer. Bei Tamandua kreuzen zwar auch die IX-Fasern in der Gegend des Crus ascendens des VII-Nerven, aber hier bleibt die Kreuzungsstelle der VII-Wurzel ganz rostral, so dasz eine deutliche Lücke zwischen den beiden Kreuzungen übrig bleibt.

Die Tatsache, dasz die kreuzenden IX- und X-Fasern zuweilen eine ununterbrochene Schicht bilden, weist darauf hin, dasz beide zum selben Fasersystem gehören, wie denn auch der Speicheldrüsenkern ein einheitliches Gebilde ist.

Oben wurde dargelegt, dasz die VII-Kreuzung m. E. nicht ausschlieszlich

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[1]) Ich führe nur die Genusnamen auf, die Arten sind aus der Liste S. 4 u. 5 zu ersehen.

aus Speicheldrüsenfasern zusammengesetzt ist, ja dasz sie bei den Cetaceen sogar ganz aus autonomen Fasern anderer Natur bestehen musz. Dies wird auch auf die IX-Kreuzung zutreffen.

Wie aus der obigen Zusammenstellung hervorgeht, kommt die IX-Kreuzung auch den Cetaceen und Pinnipediern zu. Sie ist hier sogar wohl ausgebildet. Nun fehlen, wie schon erwähnt, den ersteren Speicheldrüsen gänzlich, während bei den letzteren die Parotis stark rückgebildet ist. Demzufolge musz bei den Cetaceen die IX-Kreuzung ganz, bei den Pinnipediern teilweise aus Fasern anderer Art bestehen. Dies schlieszt aber die autonome Natur dieser Kreuzung nicht aus. SIMONETTA (1930) wies beim Schaf nach, dasz der IX-Nerv neben der Parotis auch die Schleim- und serösen Drüsen innerviert, die in groszer Zahl auf dem hinteren Teil der Zunge vorkommen. Es mag sich bei den genannten Wassersäugern um solche Fasern handeln, und die Annahme liegt nahe, dasz sie auch bei den übrigen Säugern neben Speicheldrüsenfasern in der IX-Kreuzung anwesend sein werden.

Vögel.

Für die Vögel hat ADDENS (1933, '34) an CAJAL-Präparaten eines siebentägigen Hühnchenembryos dargetan, dasz die hier von CAJAL (1909*a*) beschriebenen, auf der Höhe ihres Eintritts kreuzenden sensiblen IX- und X-Fasern nichts anderes sind wie Komponente der dorsalen Cochleariscommissur, also kreuzende sekundäre Cochlearisfasern. Auch kreuzende motorische IX- und X-Fasern konnte ADDENS in seinen Präparaten nicht auffinden. Ich kann diese Beobachtungen nur bestätigen, weder in CAJAL-Serien von Hühnchenembryonen noch in WEIGERT—PAL-Serien mancher erwachsenen Vögel gelang es mir die Anwesenheit kreuzender sensibler oder motorischer IX- oder X-Fasern nachzuweisen.

Für die VII-Wurzel liegen die Sachen anders. In der ersten Auflage seines Lehrbuchs (1893) hat VAN GEHUCHTEN auf Grund von GOLGI-Präparaten eine partielle VII-Kreuzung für das Hühnchenembryo angegeben, später aber (1898, und in seinem Lehrbuch von der dritten Auflage, 1900, ab) diese Angabe widerrufen, da er nachgewiesen hatte, dasz Durchschneiden oder Ausziehen der ganzen VII-Wurzel beim Kaninchen Chromatolyse in allen Zellen des homolateralen VII-Kernes hervorrief, während die des kontralateralen Kernes alle normal blieben.

Im Jahre 1909 beschrieb CAJAL (1909*a*) aber von neuem kreuzende VII-Fasern beim Hühnchenembryo. Vorher hatte er schon solche Fasern bei der neugeborenen Maus beobachtet, und dieselben als motorisch aufgefaszt, aber jetzt, wahrscheinlich unter dem Einflusz der experimentellen Befunde VAN GEHUCHTENS, deutet er sie sowohl bei Säugern wie bei Vögeln als sensibel. Neuerdings hat ADDENS (1934) in seiner schon öfters angeführten Uebersicht das Vorhandensein dieser Fasern bei einem siebentägigen Hühnchenembryo bestätigt. Er hält sie aber für das Homologon

der bei Säugern angetroffenen Fasern, also autonomer Natur, eine Meinung, der ich beipflichten möchte.

Auch ich fand dieses kreuzende Bündel in CAJAL-Präparaten von Hühnchenembryonen verschiedenen Alters, aber auszerdem gelang es mir dasselbe bei zahlreichen erwachsenen Vögeln nachzuweisen (Fig. 4—6).

Wie bei den Säugern überschreiten die kreuzenden Fasern hier die Medianebene dorsal vom medialen Längsbündel, treten aber nicht mit dem Vestibularis, sondern mit ihrer eigenen Wurzel aus. Wo ihre Ursprungszellen liegen kann ich nicht angeben.

Für meine Untersuchung standen mir WEIGERT—PAL-Serien, fast

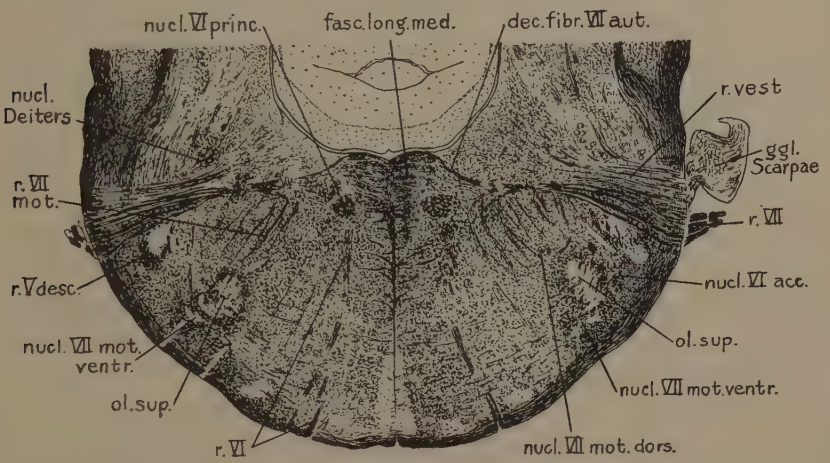

Fig. 4. *Catharistes urubu* (Rabengeier). Querschnitt durch die Kreuzung der autonomen VII-Fasern. WEIGERT—PAL-Parakarmin.

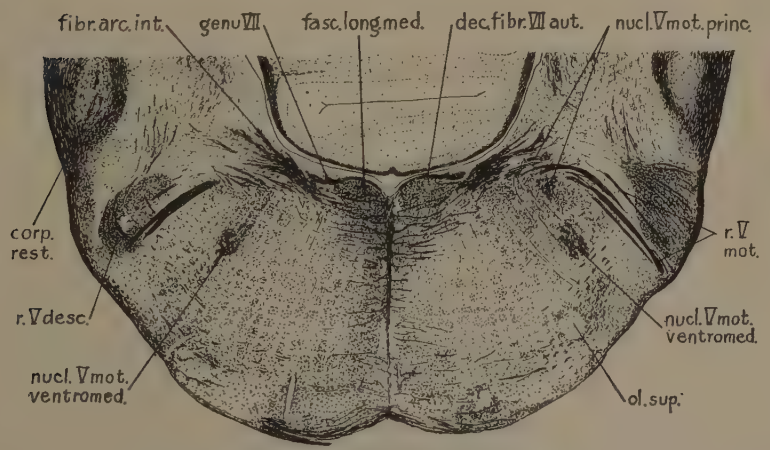

Fig. 5. *Casuarius australis.* Querschnitt durch die Kreuzung der autonomen VII-Fasern. WEIGERT—PAL-Parakarmin.

durchwegs nachgefärbt mit Para- oder Alaunkarmin von den folgenden Vögelarten zur Verfügung: Ratiten: Casuarius australis, Rhea

americana, Struthio spec.; Tauchvögel: Colymbus septentrionalis, Podiceps cristatus; Pinguine: Spheniscus demersus; Storchvögel: Botaurus stellaris, Ciconia alba, Phoenicopterus roseus; Gänsevögel: Cygnus olor; Raubvögel: Buteo vulgaris, Catharistes urubu, Geranoaëtes melanoleucus, Sarcorhamphus papa; Hühnervögel: Huhn, Lophortyx californicus; Kraniche: Grus japonensis; Möwen: Larus argentatus; Tauben: Taube, Columba guinea; Papageien: Agapornis nigrogenis, Amazona oratrix, Cacatua roseicapilla, Chrysotis amazonica, Lorius garrulus, Melopsittacus undulatus, Palaeornis torquata; Eulen: Athene noctua, Strix flammea; Kolibris: Lampornis mango; Pfefferfresser: Rhamphastos dicolurus; Sperlingsvögel: Certhia brachydactyla, Chrysomitris spinus, Emberiza citrinella, Erithacus rubecula, Ligurinus chloris, Motacilla alba, Munia orizivora, Parus major, Passer domesticus, Sturnus vulgaris.

Bei der Mehrzahl obiger Arten konnte ich die VII-Kreuzung feststellen, nur bei den folgenden gelang mir dies nicht: Athene[1]), Chrysotis, Lophortyx, Lorius, Motacilla, Parus, Phoenicopterus, Podiceps, Rhamphastos, Sturnus. Doch möchte ich nicht glauben, dasz die kreuzende Wurzel hier fehlt. Nur in der Minderzahl der Fälle, wie z. B. bei den hier abgebildeten Casuar, Sperling und Rabengeier, ist das Bündel gut fasciculariesiert, meistens sind seine Fasern aber locker gefügt, wodurch es schwierig zu verfolgen ist. So halte ich es für sehr wahrscheinlich, dasz weniger gute Färbung oder sonstige Umstände Ursache des Nichtauffindens des Bündels sind, und dasz es allen Vögeln zukommt. Das Zerstreutsein der Ausnahmen über fast sämtliche Gruppen, wo nahe verwandte Formen die Kreuzung wohl aufweisen, weist darauf hin.

Beim Hühnchenembryo liegt die Kreuzung ungefähr in der Austrittsebene der Wurzel, bei weitaus den meisten erwachsenen Vögeln (z. B. Casuar, Fig. 5; Sperling, Fig. 6; auch beim erwachsenen Huhn) liegt sie aber weit vor dieser Stelle, in nur drei Fällen (Catharistes: Fig. 4, Rhea und Struthio) fand ich sie in der Austrittsebene der Wurzel.

Unter den Arten, wo die Kreuzungsstelle vor der Austrittsebene der Wurzel liegt, zeigen die Papageien und Sperlingsvögel eine interessante Komplikation. Bei Cacatua roseicapilla hat ADDENS (1933) beschrieben, dasz ein Teil der Fasern aus dem dorsalen VII-Kerne nicht mit dem Rest der VII-Wurzel austritt, sondern sich der motorischen V-Wurzel anschlieszt. So gibt es hier zwei VII-Wurzeln, eine schwächere vordere, die mit der V-Wurzel austritt, und eine stärkere hintere. Dies ist ein Beispiel der von ADDENS begründeten Lehre der zentralen Anastomose, welche besagt, dasz die Nerven keine feste Einheiten sind, sondern dasz sich innerhalb des Zentralnervensystems ein Teil eines Nerven einem anderen anschlieszen kann, wodurch der falsche Eindruck erweckt wird, dasz er zum letzteren gehöre. Der Nachweis einer derartigen doppelten VII-

[1]) Vgl. für die Speziesnamen die Liste.

Wurzel gelang mir bei fast allen untersuchten Papageien und Sperlingsvögeln. Unter den ersteren konnte ich sie nicht bei Chrysotis und Lorius, unter den letzteren nicht beim Star feststellen, was wahrscheinlich der weniger guten Färbung der Serien zuzuschreiben ist.

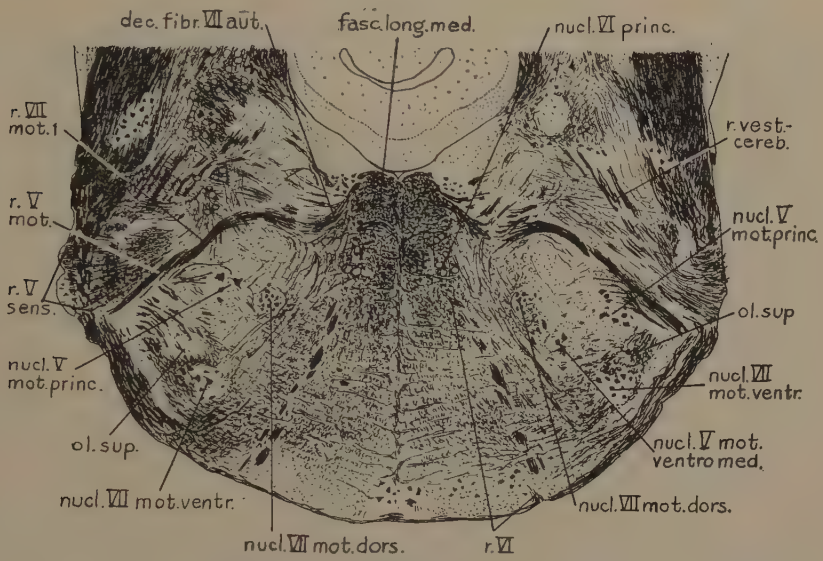

Fig. 6. *Passer domesticus.* Querschnitt durch die Kreuzung der autonomen VII-Fasern. WEIGERT—PAL-Parakarmin. r. VII mot. 1, vordere motorische VII-Wurzel (vgl. Text).

Im Falle einer doppelten VII-Wurzel findet die Kreuzung der autonomen Fasern meistens in der vorderen Wurzel statt. Bei Certhia und Chrysomitris kommt sie aber in beiden Wurzeln vor. Bei Ligurinus scheint es mir, dasz das kreuzende Bündel zwischen der linken vorderen und rechten hinteren Wurzel schief passiert. Bei Motacilla und Parus waren keine kreuzende Fasern nachzuweisen, wie schon oben angegeben.

In Anbetracht der Tatsache, dasz ich eine doppelte VII-Wurzel nur bei den Papageien und Sperlingsvögeln antraf, musz hier ein Befund NAKAMURAS (1930) erwähnt werden, der so gedeutet werden kann, als ob sich auch anderwärts solche Verhältnisse finden. In seiner Arbeit beschreibt der Autor was er für den accessorischen VI-Kern hält bei einer Anzahl von Vögeln. Wie ADDENS (1934, S. 317) aber darlegte, hat NAKAMURA den wirklichen accessorischen VI-Kern gar nicht gesehen, sondern den dorsalen VII-Kern dafür gehalten. Für den Flamingo (Phoenicopterus roseus) gibt NAKAMURA an: „Im Trigeminusgebiet ziehen von diesem Kern aus Fasern zur Trigeminuswurzel". Man könnte nun denken, dasz es sich hier um eine vordere VII-Wurzel handelte. Dem ist aber nicht so. Die Durchsicht einer WEIGERT—PAL-Parakarmin-Serie eines Flamingos hat mir ergeben, dasz es bei diesem Vogel ein ventromedialer Abschnitt des V-Kernes gibt, der ungefähr in der Verlängerung

des dorsalen VII-Kernes liegt. Hier hat also NAKAMURA den dorsalen VII-Kern samt dem ventromedialen V-Kern als accessorischen VI-Kern angesprochen. Das von ihm beschriebene und abgebildete Faserbündel (Fig. 9, S. 280 seiner Arbeit) ist ein gewöhnliches V-Bündel.

Reptilien.

Bei den Reptilien sind meines Wissens bisher weder in der VII- noch in der IX-Wurzel kreuzende Fasern beschrieben worden.

Die Untersuchung von WEIGERT—PAL-Serien mehrerer Vertreter aller vier Hauptgruppen: Schildkröten, Krokodile, Eidechsen und Schlangen hat mir ergeben, dasz hier dieselben Verhältnisse wie bei den Vögeln vorliegen.

Wohl ohne Ausnahme kommt eine kreuzende VII-Wurzel vor (Fig. 7, 8), während eine kreuzende IX-Wurzel konstant fehlt. Wie bei den Säugern und Vögeln ziehen auch hier die kreuzenden VII-Fasern dorsal vom medialen Längsbündel zur kontralateralen Seite. Sie treten, wie bei den Vögeln, mit dem Rest der Wurzel aus, niemals gesellen sie sich, wie bei den Säugern, dem Vestibularis zu.

Die Liste der untersuchten Arten ist folgende:

Schildkröten: Chelone mydas, Damonia subtrijuga, Emys orbicularis; Krokodile: Caiman sclerops, Crocodilus porosus; Eidechsen: Agama stellio, Anguis fragilis, Anolis carolinensis, Chamaeleon vulgaris, Hydrosaurus salvator, Lacerta viridis, L. vivipara, Varanus salvator; Schlangen: Ancistrodon contortrix, Boa constrictor, Python reticulatus.

Bei Anolis, Lacerta vivipara und Ancistrodon konnte ich keine kreuzenden VII-Fasern finden, doch bin ich aus den schon öfters erwähnten Gründen auch bei den Reptilien der Annahme eines allgemeinen Vorkommens der VII-Kreuzung sehr geneigt.

Meistens liegt die Kreuzung nur in der Kniegegend und vor der Austrittsebene der Wurzel (Fig. 8), aber bei allen untersuchten Schildkröten: Chelone (Fig. 7), Damonia, und Emys, sowie beim Krokodil, dehnt sie sich auch weiter nach hinten zwischen den horizontalen Schenkeln der motorischen VII-Wurzel aus, so dasz sie auch in der Austrittsebene der Wurzel liegt.

Auf Grund obiger Ausführungen scheint der Schlusz berechtigt, dasz eine kreuzende VII-Wurzel und zwar autonomer Natur, den Amnioten allgemein zukommt, eine kreuzende IX-Wurzel[1]), gleichfalls wahrscheinlich autonom, nur den Säugern.

Wie verhalten sich nun die Anamnier in dieser Hinsicht? In der Literatur findet sich meines Wissens nur eine einzige Angabe über das Vorkommen kreuzender VII-Fasern bei diesen Tieren und zwar bei den

[1]) Vgl. aber Fusznote S. 707.

Teleosteern, solche über kreuzende IX- und X-Fasern sind mir hier nicht bekannt. Persönlich habe ich nur WEIGERT—PAL-Parakarmin-Serien von Rana esculenta, R. catesbyana und Triton vulgaris, sowie einiger Teleosteer, untersucht, wo ich keine kreuzenden Fasern fand.

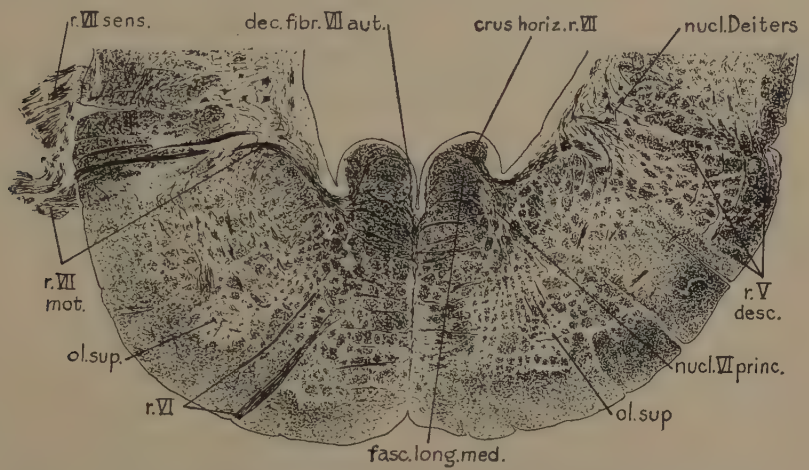

Fig. 7. *Chelone mydas.* Querschnitt durch die Kreuzung der autonomen VII-Fasern. WEIGERT—PAL-Parakarmin.

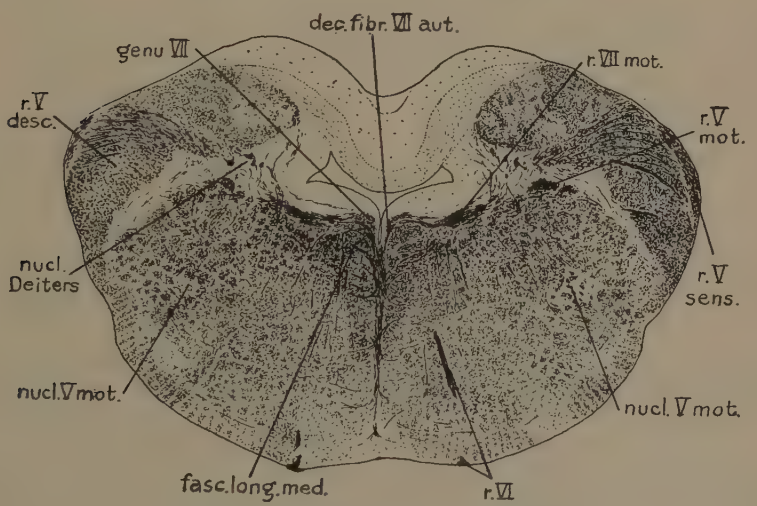

Fig. 8. *Boa constrictor.* Querschnitt durch die Kreuzung der autonomen VII-Fasern. WEIGERT—PAL-Parakarmin.

Die Angabe über Teleosteer rührt von VAN GEHUCHTEN (1892) her, der in GOLGI-Präparaten von Forellenembryonen einige motorische VII-Fasern die Medianebene überschreiten sah. In einer WEIGERT—PAL-Serie einer erwachsenen Forelle konnte ADDENS (1934) aber keine kreuzenden VII-Fasern finden. Er denkt daher, dasz sie sehr wenig an der Zahl sind oder beim erwachsenen Tier fehlen, möglicherweise auch solch einen Verlauf nehmen, dasz sie nur experimentell darstellbar sind.

Bei den Amphibien, deren Gehirn besonders durch die Arbeiten HERRICKS und RÖTHIGS zu den bestbekannten gehört, sind auch mit den Silbermethoden niemals kreuzende Fasern in den VII-, IX- und X-Wurzeln nachgewiesen worden.

So glaube ich trotz der Beobachtung VAN GEHUCHTENS, die ich nicht anzweifeln will (es mag sich hier um eine Ausnahme und um vereinzelte Fasern handeln), dasz im allgemeinen den Anamniern kreuzende Fasern in den von uns hier studierten Nerven abgehen.

Den Herren Prof. Dr. C. U. ARIËNS KAPPERS und Dr. J. L. ADDENS spreche ich meinen verbindlichsten Dank aus für den Hinweis auf dieses Thema, sowie für ihre freundliche Unterstützung bei der Ausarbeitung desselben.

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Medicine. — *On the so-called interruption of bulbocapnine-catatonia by means of a mixture of carbon dioxide and oxygen.* By H. DE JONG. (From the psychological laboratory of the Valerius clinic and the Free University Prof. L. VAN DER HORST.) (Communicated by Prof. B. BROUWER.)

(Communicated at the meeting of October 27, 1934).

Experimental catatonia appeared to us[1]) to be a frequently occurring form of reaction of the central nervous system. First we have, together with BARUK[2]), described the syndrome of experimental catatonia for the bulbocapnine reaction. Later on we observed that many other substances may produce catatonia within part of their dosage area. The electric current also appeared to be able to produce such actions under certain circumstances. From our experiments on asphyxial catatonia[3]) it became apparent that carbon dioxide can also produce catatonic phenomena in some experimental animals, namely in mice. The mouse is put in a test glass, into which CO_2 is introduced; the animal is affected with dyspnoea and after a while she shows clonic-tonic contractions. The animal is now taken into the

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1) H. DE JONG: "La catatonie expérimentale comme réaction fréquente du système nerveux, etc.". Annales médico-psychologiques. N°. 2, 1933.
2) ———— et H. BARUK: "La catatonie expérimentale par la bulbocapnine". Paris, Masson 1930.
3) ————: "La catatonie expérimentale par des substances gazeuses". Psych. en Neur. Bl. Nr. 3/4, 1934.

fresh air and in some cases a stage may be observed, in which fine complex hyperkinesiae dominate the picture. At the slightest stimulation the mouse takes great jumps, revolves round her longitudinal axis, etc. Then a stage of normal motility appears, followed by a fine catalepsy. The animal may be hung on a thread by two legs, without erecting herself; placed on a bar she stands immovable, even if she has her tail pinched several times. The whole syndrome of experimental catatonia thus develops in reverse order to that of catatonia as it is obtained by means of injection, for example, with bulbocapnine. Here we find: first catalepsy and then, upon increase of the dosage, hyperkinesiae, changing upon still further increase of the dosage into tonic-clonic convulsions (epilepsy). Autonomic phenomena, (an accelerated respiration occurring also in CO_2-catatonia,) complete the picture.

KAUFMANN and SPIEGEL [1]) now describe that bulbocapnine-catatonia in cats is interrupted during 5—10 minutes, if the animals are made to inhale a carbon dioxide-oxygen mixture. Their experiments were inspired by the work of LOEVENHART, LORENZ and WATERS [2]) on what these authors termed "cortical stimulation" by administration of CO_2, for example, to stuporous patients. A combination with oxygen took place; first with 10—15 % CO_2; the respiration was then accelerated, and subsequently the CO_2 dose was increased with 5 % each minute until a concentration of 30—40 % had been reached. The stuporous patients were said to wake up from their stupor during 2—20 minutes, which, among other things, became apparent by their starting to talk in that time.

SOLOMON and KAUFMANN [3]) improved the method and noted the above-mentioned action chiefly in the manic-depressive stupor, sometimes in the schizophrenic stupor as well.

We have now raised the question how this action may be explained. A carbon dioxide-oxygen mixture would be able to interrupt a stuporous state in man, and in the experiment on animals the same might occur in bulbocapnine-catatonia.

In a series of experiments on mice, rats, cats, and a monkey we have now in the first place tried to find out whether we have to deal here with an action of the oxygen or of the carbon dioxide. We have namely brought forward a theory of cellular asphyxia [4]) as one of the possible explanations of the catatonic syndrome. It might be that it is the oxygen which acts here as a direct antagonist. The second possibility is that the carbon dioxide exerts the above-mentioned action. It would be necessary then at the same

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[1]) M. R. KAUFMANN and E. A. SPIEGEL: "Zeitschr. f. die ges. Neur. u. Psych. Bd. **127**, 1930, S. 312.

[2]) A. S. LOEVENHART, W. F. LORENZ and R. M. WATERS: Journ. of Amer. Med. Assoc. **92**, 1929, p. 880.

[3]) SOLOMON and KAUFMANN: Proc. Boston Soc. Psychiatr. May 1929.

[4]) Katatonie als motorisch physiologisch grondphenomeen op de basis van cellulaire asphyxie. Ned. T. v. Gen. 1933, bl. 2144—2145.

time to explain how carbon dioxide, which itself can produce catatonic phenomena, can interrupt, for example, bulbocapnine-catalepsy.

A first series of experiments shows the phenomenon of a temporary occurrence of normal motility, if an animal, made cataleptic with bulbocapnine, is treated with CO_2 and O_2 (50 % of each). A protocol of such an experiment on a cat follows here:

White cat. 27th October 1933.

10.30 Injection of 100 mg of commercial bulbocapnine.

10.40 Properly cataleptic, hangs on to the edge of the table. Placed in a bell-glass; CO_2 and O_2 are supplied. After some minutes tonic-clonic convulsions occur. The animal is taken into the fresh air. She falls on her side, still twitches for some minutes, recovers and *then walks spontaneously and reactively for about 10 minutes.* When after that she is hung on the edge of the table, it appears that complete catalepsy exists again. Come down on the floor, the animal does not walk either, stands immovable, is again completely catatonic.

11.30 The animal has spontaneously recovered, begins to walk slowly. A second injection of 50 mg is given.

11.47 Is not yet so strongly cataleptic as before.

KAUFMANN and SPIEGEL had for some minutes during a similar experiment observed prompt reaction to stimuli; in our experiment also an interruption of the catalepsy occurred for a short time to such an extent that the animal walked normally, but then relapsed into catalepsy.

The last-mentioned authors further only published a protocol about cats. No experiments were published about the "antagonistic" action on other animals.

We extended our experiments also to mice and monkeys. With mice we did not succeed in demonstrating such an effect. One of these protocols runs as follows:

O_2 and CO_2.

Mice, 13th October 1933.

3 mice, 1 black and 2 white ones, are injected with 1.5 mg of phosphas bulbocapnini.

After 5 minutes a fine catalepsy.

The animals are now placed in a glass cylinder, into which CO_2 50 % and O_2 50 % have been introduced.

The animals become unconscious, are dyspnoeic.

After recovery they are *just as cataleptic as before.*

Conclusion: No influence of the CO_2/O_2 mixture.

With the monkey the results were as follows:

17th November 1933. Monkey of 3.5 Kg (macacus cynomolgus).

11.20 Injection of 75 mg of phosphas bulbocapnini.
11.30 Is not yet completely cataleptic; walks spontaneously.
11.34 Is now completely cataleptic. The animal is placed in a bell-glass.
11.40 A mixture of O_2 and CO_2 (c. 50 % of each) is introduced.
11.43 Some convulsions occur; the animal is at once taken out of the bell-glass, *spontaneously takes a jump.*
11.45 Is again completely cataleptic.
11.50 The same experiment is repeated.
11.55 Clonic twitchings occur. The animal lies on his side and is taken out of the bell-glass; he exhibits a "crucifixion attitude", drops off his legs, *takes a few steps.* Immediately afterwards the animal is again negativistic, resists being pushed forward, after which, however, a few steps follow again.
12.05 Placed on the table, the animal offers resistance, *jumps from the table,* but after coming down he stands cataleptically with one leg fixed against the edge of the table. Salivation and flexed attitude are distinctly present; sometimes there is a sligt tremor of the head.
12.10 Is entirely immovable in the posture of the "Penseur de Rodin". When pinched in the tail, he still turns round for a moment.
12.15 Reacts no longer to pinching in the tail.
Conclusion: The catalepsy has been temporarily interrupted here.

The following protocol shows that great care should be taken not to draw hasty conclusions in case of the monkey. At a low dosage of bulbocapnine the catalepsy in this experimental animal is namely more transient than in cats at a corresponding dose. The catalepsy may then be interrupted by other stimuli as well:

The following is the relevant protocol, when the animal was excited by being put in the narrow bell-glass.

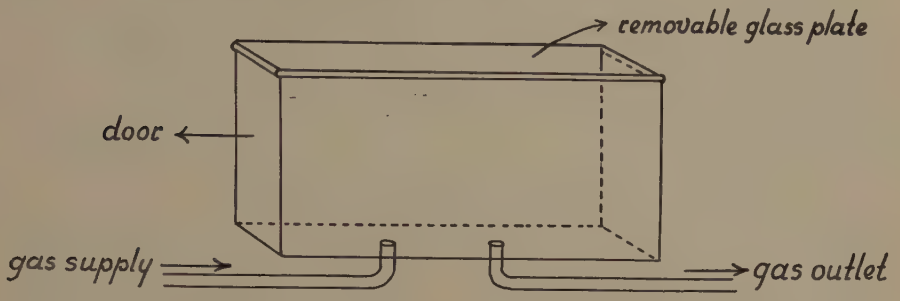
10th November 1933. *Experiment on a monkey, injected with bulbocapnine, with a mixture of* CO_2 *and* O_2.

Macacus rhesus, body weight 3.4 Kg.

10.44 Injection of 50 mg of phosphas bulbocapnini in a long dorsal muscle.
10.54 A fine catalepsy with salivation and occasionally a very slight tremor of the head.
11.00 is placed in the bell-glass, is then very negativistic.
11.04 The introduction of an equal mixture of CO_2 and O_2 is on the point of being started, but the animal is first taken out again for a demonstration of the catalepsy and appears now to have a practically normal motility. The animal runs off but, left alone, he sits down with strongly crooked back. *The motility is practically normal.* The experiment is now discontinued. Not until after ten minutes the catalepsy is restored.

The amount of bulbocapnine, which had been injected, was evidently too small. In order to prevent excitation, due to putting in a narrow bell-

glass, the monkey was placed during further experiments in a special cage, a section of which, speaking for itself, is here reproduced.



A. Experiments on a normal monkey.

First the action of carbon dioxide only, on the normal monkey is investigated. The result appears from the following protocols:

Short action of CO_2.

8th December 1933. Monkey, macacus rhesus, 3.4 Kg, is placed in the glass cage.

CO_2 is supplied.

After about five minutes the animal is affected with dyspnoea, takes a few jumps, is taken out into the fresh air and there almost immediately regains his normal motility.

Conclusion: Dyspnoea, then normal motility during recovery.

Then the CO_2 *is introduced for a longer time*:

10.45 Monkey, macacus rhesus, 3.4 Kg. CO_2 is supplied for a longer time.
10.50 The animal drops off his legs; shows the crucifixion attitude, lying prostrate with the forelegs across on the floor.
10.52 The animal, highly dyspnoeic, falls on his side.
10.53 When he is taken out into the fresh air, his legs appear to be entirely flaccid. The animal also shows salivation.
10.56 The monkey recovers quickly, gets into a stage of excitation, takes great jumps, runs about the room, jumps against the window panes.
11.03 The motility now becomes normal and remains in that condition.

Conclusion: Abnormal postures, finally narcotic condition, paresis. When the animal was taken into the fresh air, first an excitation-stage occurred with hyperkinesia and afterwards normal motility.

Then the experiment was repeated. At first, however, a mixture of *carbon dioxide and oxygen* was administered to the normal monkey. The animal only showed accelerated respiration and no further effect. The introduction of oxygen was now discontinued, so that again only carbon dioxide was administered.

For a short while paresis in the legs was observed, then epileptiform

attacks with tonic-clonic convulsions. When the animal was taken into the fresh air, enormous hyperkinesiae manifested themselves in the form of great jumps. After that the motility became normal again.

In the third place the effect of *oxygen only* on the normal monkey was investigated.

22nd December 1933. Monkey, macacus rhesus, 3.4 Kg.

10.35 Motility normal.
Is placed in a glass cage. *Pure ogygen* is introduced.

10.55 The motility of the animal remains normal.

B. Then *experiments* were made *on the monkey, injected with bulbocapnine.* First the effect of *oxygen only* during the cataleptic stage was investigated :

22nd December 1933.

11.00 Injection of 75 mg of phosphas bulbocapnini.

11.10 Highly cataleptic and negativistic ; salivation, occasionally a slow tremor. Then oxygen is administered.

11.30 The animal remains completely in hypokinetic-catatonic condition.

Conclusion : Oxygen only does not affect the cataleptic condition of the monkey.

Secondly the effect of *carbon dioxide only* on the bulbocapnine-monkey was investigated :

12th January 1934. Monkey, macacus rhesus, 3.4 Kg.

10.40 Injection of 80 mg of phosphas bulbocapnini.

10.50 Completely cataleptic and strongly negativistic, with salivation.

10.55 Introduction of carbon dioxide.

10.58 Tonic-clonic convulsions occur.

11.00 Taken out of the cage, the animal shows spreading of the forelegs (crucifixion attitude), the hind legs lying slack by the side of the body.

11.15 The animal is again normally cataleptic, in side position as well as standing.

Conclusion : Carbon dioxide occasioned paresis and tonic-clonic convulsions, superposed upon the cataleptic condition. Normal motility was not observed.

Thirdly we investigated the effect of a *carbon dioxide-oxygen mixture* (50 % of each) on the cataleptic monkey.

Monkey, macacus rhesus, 3.4 Kg. 19th January 1934. Before the experiment it is ascertained that the motility is perfectly normal.

10.45 Injection of 75 mg of phosphas bulbocapnini.

11.00 Complete catalepsy with negativism, salivation, etc. The monkey is placed in the glass cage.

11.05 Introduction of the mixture of CO_2 and O_2 in the cage (50 % of each).

11.10 The animal drops off his legs in crucifixion attitude.

11.12 Sudden occurrence of violent tonic-clonic convulsions.
11.15 The animal is taken out of the glass cage into the fresh air.
11.25 The convulsions decrease. The animal is lying on his side with the hind legs under the body and the forelegs across on the body.
11.45 The animal becomes gradually stronger and again exhibits the same catalepsy as before the introduction of the carbon dioxide-oxygen.

Conclusion: Paresis and epilepsy, superposed on bulbocapnine-catalepsy by O_2 and CO_2. No normal motility is observed.

This experiment was repeated three times on other dates, always with the same result.

Finally we here mention the result of some *experiments in 2 cases of human catatonia, where a mixture of CO_2 and O_2 had been administered.* In both cases no effect whatever on the catatonic mechanism was to be perceived. We here insert the protocol of one of these experiments (repeated several times with either patient):

15th March 1934.

Patient X. Dementia praecox. Lies in bed in a stuporous condition. Slightly cataleptic and negativistic. Does not reply to questions. A mixture of carbon dioxide and oxygen in the ratio of 50 %—50 % is administered by means of a large funnel, placed on the face and connected with a tube which via a *T*-shaped piece runs to the gas-cylinders. After a quarter of an hour and half an hour respectively the condition is still unaltered, i.e. no answer is given to questions.

22nd March 1934.

The same experiment is repeated. The patient is now more in an excitation stage; he lies quietly in bed but at the slightest stimulation he gets up, shouts and tries to fight. With the help of some assistants he is fixed. Now the mixture of CO_2—O_2 is supplied. After 20 minutes the condition is still exactly the same.

Conclusion and summary: Our series of experiments, carried out for many years, concerning experimental catatonia, made us also consider the problem of a possible anti-catatonic action.

In the first place we investigated in how far a mixture of CO_2 and O_2 might exert a really antagonistic influence on catatonic conditions.

Communications in the literature on the interruption of a stupor during 2—20 minutes by CO_2 and O_2 (LOEVENHART, LORENZ, WATERS, SOLOMON, and KAUFMANN), in addition to a similar action in case of bulbocapnine-catatonia in cats (KAUFMANN and SPIEGEL) led us to this investigation.

In two patients we did not find any effect on the catatonic condition. In case of bulbocapnine-catatonia of some cats we could for some minutes observe some normal walking movements after administration of a

CO_2—O_2 mixture, in the monkey in the greater part of numerous experiments only a mixture of hyperkinesiae, superposed on the bulbocapnine-catalepsy. In one case only we noticed some normal walking movements after administration of CO_2—O_2 to a cataleptic monkey. We also could point out by means of systematic research that CO_2 and not O_2 is the agent which alters the motility, while CO_2 and the mixture of CO_2 and O_2 in monkeys both appeared to provoke the same hyperkinesiae, superposed on the bulbocapnine-catalepsy.

In previous investigations, particularly on mice, we have been able to show that in these experimental animals CO_2 also can produce the complete catatonic syndrome, i.e. catalepsy as well as hyperkinesia.

Our final conclusion now is that in case of the action of CO_2 on bulbocapnine-catalepsy we have not at all to deal with a real antagonism to bulbocapnine.

The same hyperkinesiae, shown by the bulbocapnine-monkey under the influence of CO_2, are also exhibited by this experimental animal if CO_2 is supplied without bulbocapnine. Moreover, we pointed out before that bulbocapnine itself, at a higher dosage than is required to cause catalepsy, also provokes hyperkinesiae, which hyperkinesiae perfectly agree with those caused by CO_2-administration.

It is besides a well-known fact that the dose in between the one causing catalepsy and the one producing hyperkinesiae may sometimes occasion a state of equilibrium between catalepsy and hyperkinesia, in which case the motility is normal. The influence of CO_2 and of the mixture of CO_2 and O_2 respectively may, in our opinion, be put on a level with this.

The gas mixture thus does not act as an antagonist to bulbocapnine, but as bulbocapnine itself upon administration of a larger dose than the one producing catalepsy.

Neurology. — *Preliminary Investigation concerning the Representation of the Fovea in the External Geniculate Body of the monkey.* By S. BRODY, M.D. Yale University, New-Haven. (From the neurological and ophthalmological laboratories of the University of Amsterdam). (Communicated by Prof. B. BROUWER).

(Communicated at the meeting of October 27, 1934).

I. *Introduction.*

A panorama of the development of our present day knowledge of the anatomy of the visual system can be obtained by reference to the work of VON MONAKOW (10), HENSCHEN (7), MINKOWSKI (9), WINKLER (13), RÖNNE (12), BROUWER and ZEEMAN (2), POLJAK (11), DEUTSCH (5), HECHST (6), BALADO and FRANKE (1), CLARK and PENMAN (4) and others.

The impetus which was derived from the experimental research of BROUWER and ZEEMAN and their collaborators in the analysis of the opto-geniculate radiations has stimulated considerable investigative work.

As early as 1914, RÖNNE (12), was able to demonstrate the projection of the macula on the external geniculate body of man, in cases of toxic amblyopia. He found the degeneration to be confined to the dorso-medial portion of the geniculatum. From his work, no inferences were possible as to the details of this projection, because the lesions were bilateral. On the experimental side, the work of BROUWER and ZEEMAN (2), first established some insight into the macular radiation in monkeys. With the aid of the Marchi method, they found in the monkey that the dorsal quadrants of the retina were projected medially, the ventral quadrants laterally in the geniculatum; and that the macular field occupied a rather large portion of the dorso-medial part of the geniculatum lying between and partly overlapping the fields belonging to the peripheral sections of the retina. The authors believed that this was an apparent rather than a real overlaps, and felt that they had reached the limit of accuracy with the Marchi-method.

In 1934 an excellent paper by CLARK and PENMAN (4) appeared describing the distribution of the atrophy in the geniculate body following experimental lesions made in the retina and macula of monkeys. They found a complete decussation of the fibres arising in the nasal halves of the maculae and confined the macular area to a median sector in the posterior two-thirds of the nucleus. In only one instance was an isolated injury of the macula obtained, however.

The ability to solve the question of the macular representation depends on either destroying the macula in toto, or on making partial lesions and summating the resultant fragmentary areas of degeneration in the geniculatum into a composite scheme. The present communication is a study of the secondary degeneration in the geniculate body subsequent to a small precise lesion in one macular area of a monkey.

II. *Experimental method.*

Operation, July 5, 1933, (Prof. Dr. W. P. C. ZEEMAN), sacrificed, Dec. 1, 1933. The technique used in producing a macular lesion was described in a publication by BROUWER and ZEEMAN (2). The macaque was used for this experiment and a complete series of frontal sections through the geniculate bodies of another normal macaque was made for control purposes. The animal was killed 5 months after operation by an overdose of anaesthetic. Just before it was sacrificed, the fundi were examined and a photograph was made of the macular lesion. The brain was removed and put into 10 % acid-free formalin for twenty hours. Blocks containing the optic nerves and chiasm were then excised for examination with Weigert-Pal and van Gieson coloration. The geniculate bodies, pulvinar, and anterior quadrigeminate bodies were cut out en bloc and were stained by the Nissl technique. A complete frontal series of both blocks was made at

a thickness of 30 microns, and every section was stained. The left eye was sectioned serially and stained with hematoxylin-eosin in order to study the lesion in the macula itself. The geniculate bodies of the normal animal were also imbedded in celloidin and colored by the Nissl method.

III. *Description of the conditions of the retina.*

The lesion which was made in the macula under control of the ophthalmoscope was photographed some days after the operation (cf. Figure I).

The histological examination shows, that the lesion is circumscript. The incision was made immediately above the centre of the fovea and was limited to the temporal half of the macula. Adjoining the temporal border of the incision the layer of ganglion cells is diminished in width but it still contains so many cells that the temporal upper quadrant of the macula cannot possibly have lost its function completely, even though part of the fibres originating there must have been cut at the operation.

Figure II (section 240) represents a transsection through the *center* of the fovea, showing no lesion. Figure III (sections 245—266) exposes a circumscript foveal lesion, slightly to the temporal side. In section 270 no more lesion is to be seen. (Figure IV). The length of this short vertical incision in the upper half of the fovea (which undoubtedly has avoided the nasal half) corresponds to 21 sections, each 20 μ thick, thus measuring 0,42 mm. (total length).

Assuming that this incision has not only destroyed the ganglion cells in place but has also damaged all the "related" macular fibres (Figure V) the following calculation concerning the extent of the excluded area can be made. Since the radius of the macula measures 520 μ and the length of the incision 420 μ, the surface of the excluded area may be estimated to be $1/4\ \pi\ (520\ \mu)^2 - 1/4\ \pi\ (100\ \mu)^2 = (450\ \mu)^2$.

According to ZWANENBURG in the fovea 75 cones are to be found over a distance of 270 μ, corresponding to 125 cones over a distance of 450 μ. In our case therefore $125^2 = 15625$ cones have been excluded.

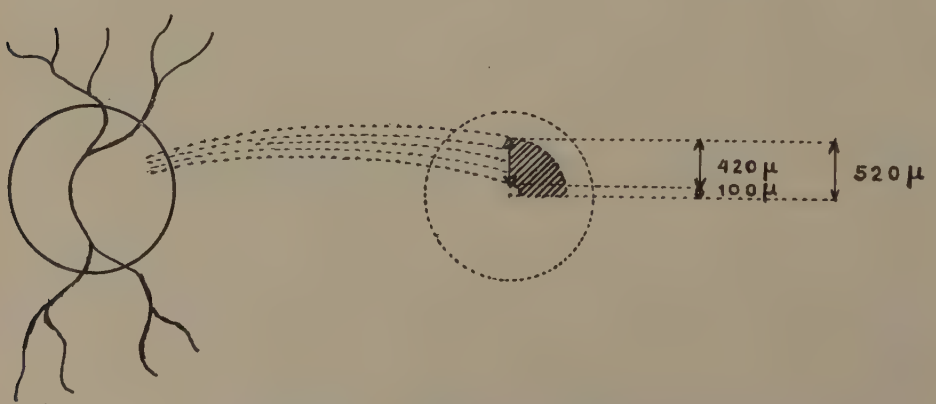


Fig. V. The extent of the excluded area.

Figure I

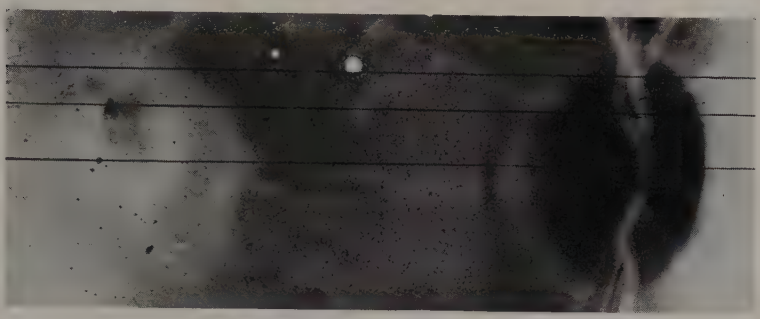


Figure II

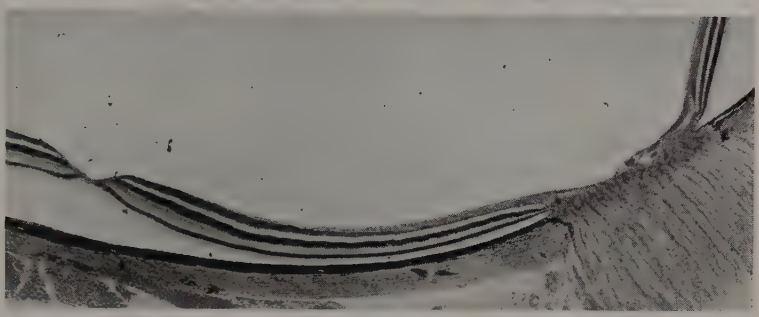


Figure III

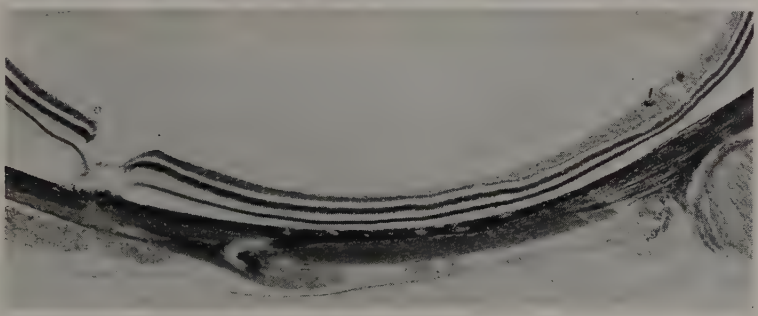


Figure IV

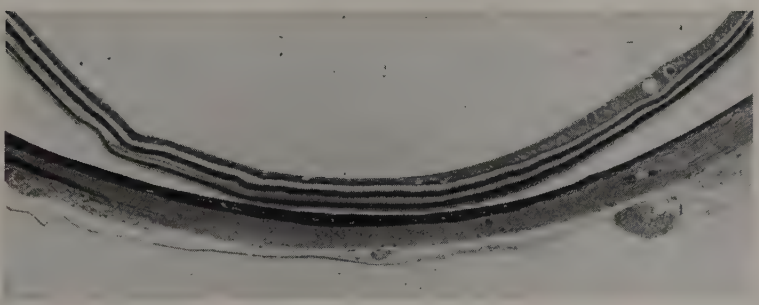


Fig. VI. Section from the geniculatum (2nd quarter) of a normal macaque showing characteristic 6 layer structure. Nissl.

Since the total number of ganglioncells equals approximately the total number of cones, i.e. ± 4.000.000, such part of the retinal ganglioncells would have been excluded as corresponds to $\frac{15625}{4000000} = 1/256$. This should be considered as a maximal estimation of the damage caused by the lesion [1]).

IV. *The morphology of the lateral geniculate body in macacus rhesus.*

The anatomy of the geniculatum in macacus rhesus has been well described in publications by MINKOWSKI (9), and CLARK, and the striking resemblance to the human geniculatum was noted. For the sake of convenience in description, the geniculatum can be considered as divided into 4 quarters in an orocaudal direction. The middle 2 quarters present the characteristic 6 layer structure (Fig. VI) and they are designated as 1 to 6. Layers 1 and 2 are the basal large cell layers, and 3, 4, 5, and 6 are the medium-sized cell dorsal layers. MINKOWSKI referred to layers 1 and 2 as the pars magno-cellularis and to layers 3, 4, 5 and 6 as the pars mediocellularis. He also showed that layers 1, 4, 6, received crossed, and laminae 2, 3, 5, uncrossed fibres from the retina.

CLARK confirmed these findings, and also found the same specifications for the macular fibres. There is a third type of small, triangular cell scattered about irregularly in the nucleus, but particularly in and around the 2 ventral layers. We were unable to find a supernumerary layer connecting laminae 3 and 5 as described by CLARK and PENMAN. However, there is no doubt that a definite structural continuity exists between all the laminae. MINKOWSKI mentioned the variability in the structural appearance of the geniculatum in different animals of the same species and even in the 2 nuclei of the same animal. In our material, the conformation of the laminae and the configuration of the nuclei were not the same in the 2 animals, but the 2 ganglia of the respective animals were an exact replica of each other in this regard. Many of the variations which have been noted depend, very likely, on the way in which the sections are cut.

V. a. *Microscopic Examination of the Geniculate Bodies and the Griseum Pregeniculatum.*

The right external geniculate body and the corpus pregeniculatum on both sides were normal. The left geniculatum (Sections 121—209) showed a very sharply defined and continuous area of cell atrophy extending from the middle of the third to the middle of the fourth quarter of the nucleus.

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1) Since according to KOSTER's older figures only 50 cones are to be found over a distance of 220 μ, only 1/400 of the total number of ganglion cells would have been excluded in our case.

In the area designated as atrophic, there was found a concentration of ganglion cells and an increase in the glia; the ganglion cells appeared shrunken and poorly stained. The atrophy was present in each of 21 successive sections (176 through 196), but only those sections were included in the diagram (Fig. VII) which were necessary to show the

Fig. VII. Schematic representation of the extent and development of the atrophy in the left corpus geniculatum.

development of the lesion. The areas in white represent advanced atrophy, while the stippled zones denote a less intense degree of atrophy. The lesion involved with varying intensity small foci in the dorso-medial

Fig. VIII. Slide 193. Note atrophy in medial extremities of layers 2, 3 and 5. Laminae 3 and united medially at this level. Nissl.

Fig. IX. Slide 193. Higher magnification showing delimited cellular atrophy. Nissl.

portions of laminae 2, 3, and 5. (Figures VIII and IX). The medial extremities of these laminae were intact except at the most caudal portion of the lesion. In each section, the abnormal areas were abruptly and clearly delimited from the intact portions of the laminae.

b. *Microscopic Examination of the Pulvinar, Oculomotor Nuclei, and the Anterior Colliculi.*

These structures revealed a normal appearance. The ganglion cells were well stained and of normal morphology. There was no rarefaction of the cellular content and no glial proliferation at any point.

VI. *Comment.*

For a proper estimation of the above results it is necessary to discuss what the macula really is, since in the current literature the word "macula" is used to cover different formations.

At first the name macula, i.e. macula lutea, was given to a central part of the retina which at post-mortem examination of the opened eye could be easily distinguished from the surrounding tissue by its yellow colour. Histologically this area is characterized by a peculiar distribution of the ganglion cells, which are absent in the centre but appear in great masses around it thus forming a definite wall. The ganglion cells increase gradually in number from the peripheral towards the central parts of the retina. The same applies to the cones. On a small area only, the rods are completely *absent*. This region has the shape of a horizontally placed ellipse and according to ZWANENBURG has a longitudinal axis of 0.44 mM (1.7°). KOSTER had already distinguished a central area, *poor* in rods, the extent of which he had calculated as 0.4—0.7 (1.7—3.4°). The difference between these two calculations has to be explained by assuming that the smaller area represents the *fovea centralis* while the larger has to be regarded as the *anatomical macula lutea*. The ophthalmoscopic macula, — that is the macula as seen with the ordinary ophthalmoscope —, however, is larger. The long axis of the elliptic area, which is distinguishable from the surroundings by a somewhat darker colour and by a totally different reflex (*macular reflex*) when looked at with the ophthalmoscope in the usual way, measures about 1.5 mM. When studied, however, with the aid of light not containing any red rays a yellow spot is visible in the centre of the retina, measuring 0.6 mM in length.

The name *physiological fovea* is also used in the literature. This is characterized by the absence of PURKINJE's phenomenon and by the deformation of the primary light ribbon, which can be observed if a light line moves along the retina.

KOHLRAUSCH gives the following figures:

Observations by FRÖHLICH:	horizontal diameter	0.63 mM	(2°25¹)
	vertical diameter	0.51 mM	(1°55¹)
Observations by VOGELSANG:	horizontal diameter	0.56 mM	(2°10¹)
	vertical diameter	0.45 mM	(1°45¹)

A comparison of the physiological and the anatomical fovea shows that both are characterized by negative qualities, namely the absence of PURKINJE's phenomenon and the absence of the rods. However, they have also positive characteristics, i. e. the maximal sensitiveness for light and the dense distribution of the cones.

It is not possible to distinguish a physiological *macula*. Although the sensitiveness for light and the visual acuity decrease considerably immediately beyond the physiological fovea, there is no peripheral border line. Towards the peripheral parts of the retina both, sensitiveness to light and visual acuity, grow *gradually* less.

In clinical literature, f. i. in the study of the central scotomas and in the so-called *"Macula-Aussparung"* the name macula is again used in a different way. One could speak of a *clinical macula*. The extent of this, when studied on the campimeter, varies according to different authors, WILLBRAND f. i. writes that it reaches 10° from the fixationpoint, VON MONAKOW from 5°—10°. This shows that the clinical macula is considerably larger than the anatomical and ophthalmoscopic macula.

It will be easier to review more correctly our own investigation after this discussion about the various conceptions of the macula.

The localisation of the area in the homolateral external geniculate body showing cell atrophy corresponds to the results, obtained by BROUWER and ZEEMAN in lesions of the macula in monkeys. The *extent* of the atrophied region, however, is smaller than could be expected from their conclusions. To explain this difference several arguments can be presented. As the atrophied area corresponds to the affected nerve-elements, it should be remembered that the number of damaged ganglion cells has been calculated as a maximum in the previous pages and therefore must be actually smaller. Secondly BROUWER and ZEEMAN used the Marchi method which exposes the pathways of the fibres in the ganglion but does not show their termination. The question whether these optic fibres entering into the external geniculate body actually get in touch with the cells (which they seem only to pass), thus enlarging the distribution of the light-stimuli, will not be discussed now. A third argument is presented by the fact that in some cases of BROUWER and ZEEMAN the lesion was made in the ophthalmoscopic macula, which as we know is larger than the anatomical area. In this light it is comprehensible why the results of our investigation approach those recently published by CARK and PENMAN.

In our opinion the *anatomical* macula should be chosen as *the* macula in future anatomical investigations of the macular projection upon the central nervous system. It is likewise preferable for clinical purposes to make use of the smaller, anatomical macula.

VII. *Summary.*

An incision was made in the temporal upper quadrant of the macular region in a monkey. Its length was determined by aid of serial sections of

the retina. The number of damaged nerve-elements was calculated by comparison with their total amount in the retina. The exact localisation of the area of secondary cell-atrophy in the external geniculate body was studied by Nissl's method. The situation of this degenerated area corresponds exactly with the conclusions of BROUWER and ZEEMAN, its extent, however, is smaller and coincides with the results of CLARK and PENMAN. An explanation of this difference is given.

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